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THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

THE RELATIONS OF ISOSTASY TO GEODESY, GEOPHYSICS AND GEOLOGY¹

WITHIN the past ten years geodetic observations have furnished positive proof that a close approximation to the condition called isostasy exists in the earth and comparatively near its surface.

Let the depth within which isostasy is found be called the depth of compensation. Think of a prismatic column which has for its base a unit area of the horizontal surface which lies at the depth of compensation, which has for its edges vertical lines, and has for its upper limit the actual irregular surface of the earth (or the sea surface if the upper end of the column is in the ocean). The condition called isostasy is defined by saying that the masses in all such columns are equal.

Fig. 1 (p. 202) represents two such columns. Column A is under the land and column B is adjacent to it under the ocean. If the condition called isostasy exists in two such columns having equal bases they have equal masses. Note that if this is true the average density in column A must be less than the average density in column B, for the volume of column A is greater than that of column B. This may be partially expressed by the statement that each excess of mass represented by material lying above sea level is compensated for by a

¹ Address of retiring vice-president of Section D (Mechanical Science and Engineering) of the American Association for the Advancement of Science, at Minneapolis, December 29, 1910, by John F. Hayford, director, College of Engineering, Northwestern University, Evanston, Ill.

defect of density and, therefore, of mass in the material in the same vertical line below sea level and above the depth of compensation.

Note that isostasy is defined in terms of masses and densities without regard to the manner in which this arrangement of masses and densities has been produced.

Isostasy is a condition of approximate equilibrium, not perfect equilibrium. The total weight of column *A* being the same as that of column *B*, the pressure at the depth of compensation due to weight is the same under the two columns, and at this level there is equilibrium. Above any selected higher level in the two columns such as that marked depth *X* in the figure, the mass is greater in column *A* than in column *B*.² Therefore, at depth *X* the pressure due to weight is greater in *A* than in *B*, equilibrium does not exist, and the material in *A* at this level tends to move downward and to the right into *B*.

The geodetic observations which have furnished a positive proof that a close approximation to the condition called isostasy exists in the earth are, first, 765 series of astronomic observations scattered over the United States from the Atlantic to the Pacific and from Canada to Mexico, and all connected by continuous triangulation.³

²The density in column *A*, in which a defect of density exists to compensate for the excess of mass at the surface, being less than in column *B*, in which the reverse condition exists, the mass in column *A* below depth *X* is necessarily less than in column *B* below that level. Hence the total masses in the two columns being equal, the mass in column *A* above depth *X* must be greater than in column *B*, as stated.

³The evidence from these observations is given in full in "The Figure of the Earth and Isostasy from Measurements in the United States" and "Supplementary Investigation in 1909 of the Figure of the Earth and Isostasy," both by John F. Hayford and both published by the Coast and Geodetic Survey.

and, second, determinations of the intensity of gravity at 89 stations scattered over the whole of the United States.⁴

The geodetic observations show that the most probable depth of compensation is 76 miles and that it is practically certain that it is not less than 62 nor more than 87 miles.⁵

Let the isostatic compensation be considered complete if in every column, such as those shown in Fig. 1, the mass above the depth of compensation is the same as in every other column. If the mass is greater or less than this in any one column, let us characterize the isostatic compensation as incomplete and measure the degree of incompleteness in terms of the excess or defect of mass.

The geodetic observations show that the isostatic compensation under the United States is nearly complete. It is not merely a compensation of the continent as a whole, it is a compensation of the separate, large, topographic features of the continent.

⁴These have furnished evidence which corroborates that from the astronomical observations and triangulation. This evidence has not been published except in brief and incomplete form (report of the sixteenth general conference of the International Geodetic Association, Vol. 1, pp. 365-389, "The Effect of Topography and Isostatic Compensation upon the Intensity of Gravity," by John F. Hayford), but it will probably be published in full within a year in a paper which is being prepared by Mr. William Bowie, inspector of geodesy, Coast and Geodetic Survey, and the speaker. It is expected that this will be published by the Coast and Geodetic Survey under the same title as the report presented at the International Geodetic Association to which reference has just been made.

⁵This is the depth of the compensation if uniformly distributed with respect to depth, which seems to be the most probable assumption. If the compensation is distributed in some other manner with respect to depth, the limiting depth of compensation is different, see pp. 77-78 of the "Supplementary Investigation in 1909 of the Figure of the Earth and Isostasy."

There is an excess of mass in some columns and a defect in others, but the evidence indicates that the average excess in the areas of undercompensation is properly represented by a stratum only 250 feet thick having the same density as the surface portion of the earth (2.67). Similarly, the average defect of mass in the areas of overcompensation corresponds to a stratum only 250 feet thick.

Contrast these small excesses and defects of 250 feet with the actual elevations in the United States, 2,500 feet on an average for the whole country. If there were no isostatic compensation these actual elevations would represent excesses of mass. The compensation may properly be characterized as departing from completeness only one tenth on an average.

These are the facts, established by abundant geodetic evidence. These facts may not be removed or altered by showing that difficulties are encountered when one attempts to make them fit existing theories geological or otherwise. The theories must be tested by the facts and modified if necessary.

A close approach to the condition called isostasy certainly exists. It is uncertain how this condition has been produced; upon that point the geodetic observations furnish no direct evidence.

The recognition of isostasy in a definite and reasonable manner in the computations of the figure and size of the earth from astronomical observations and triangulation has nearly doubled the accuracy of the computed results. This recognition, combined with other improvements in methods of computation, has enabled the Coast and Geodetic Survey to compute the equatorial radius and the flattening of the earth from observations in the United States alone with greater accuracy than it was formerly possible to compute it from

all the observations of the world combined—by such computations, for example, as those made by Bessel and Clark.

The evidence is clear that the present isostatic compensation is not an initial condition which has persisted since early geologic times. There is abundant geological evidence^{*} that within the interval covered by the geologic record many thousands of feet of thickness of material have been eroded from some parts of the United States and adjacent regions and deposited in other parts, that changes of elevation of the surface amounting to thousands of feet have been produced in this and other ways, and that these changes have continued to take place in recent time. Hence it is evident that if there had been complete isostatic compensation in early geologic time, and no readjustment toward the isostatic condition had taken place since, the departure from complete compensation would now be measured by strata thousands of feet thick upon an average. In fact, the present departures from complete compensation are measured by strata only a few hundred feet thick—250 feet on an average. It is certain that a readjustment toward isostasy has been in progress during the period covered by geologic record.

Let us consider the tendency of gravitation to produce readjustment toward isostasy. Recur to the case indicated in Fig. 1. Columns *A* and *B* have been assumed to contain equal masses. There is complete isostatic compensation. The pressures at the bases of the two columns

^{*}The paper entitled "Paleogeography of North America," by Charles Schuchert, pp. 427-606 of volume 20 of the *Bulletin of the Geological Society of America* may be cited as an example of such evidence marshaled in systematic form. Consult the fifty maps at the close of this publication for a graphic indication of the changes which have probably taken place on this continent.

are equal, and at any less depth, X , the pressure is greater in A than in B . Now assume that in the normal course of events a large amount of material is being eroded from the high surface of column A and deposited on the low surface of column B . After this erosion has been in

of the two columns were at the same level. During the process of erosion and deposition the excess of pressure in A at any level above the neutral level will continually decrease. Similarly, at any level below the neutral level the excess of pressure in B will continually increase as the

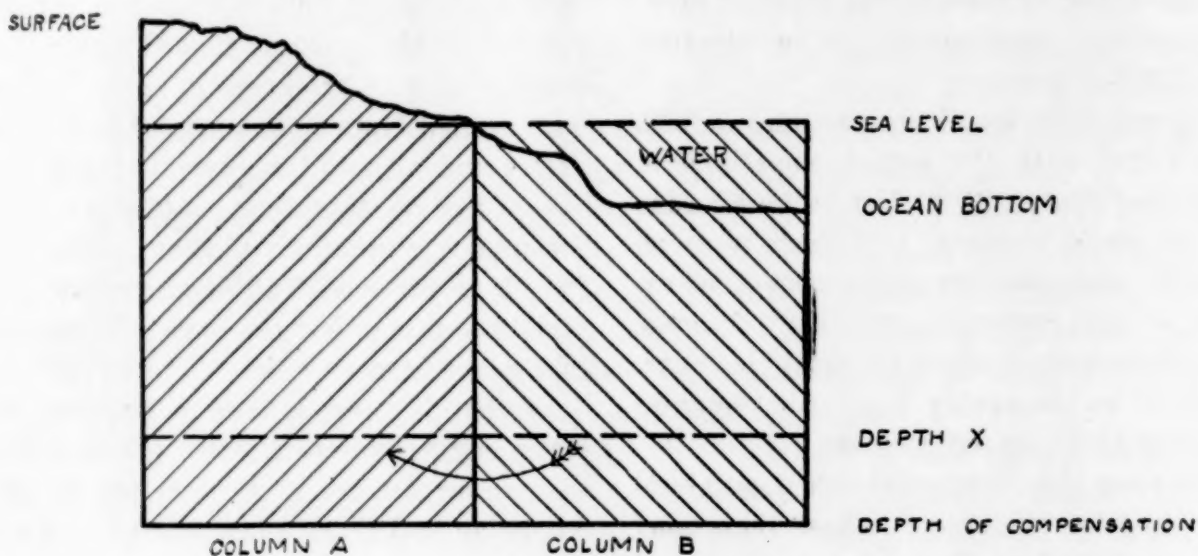


Fig. 1

progress for some time the isostatic compensation will no longer be perfect. The pressure at the base of B will be greater than at the base of A . The pressure very near the top of B will still be less than at the same level in A so long as the top of A remains higher than the top of B . There will be some intermediate level at which the pressure in the two columns is the same. Call this level of temporary equality of pressure in the two columns the neutral level. As the process of erosion and deposition progresses the neutral level will gradually progress upward from its original position at the base of the columns. Eventually if no interchange of mass took place between the columns except at the surface, and no vertical displacement occurred in either column, the neutral level would reach the surface when the process of erosion and deposition became complete and the upper surfaces

erosion progresses and the neutral level will rise. Thus there will be established a continually increasing tendency for the material below the neutral level in B to be squeezed over into A . If the stresses tending to produce this undertow from the lower part of B to A become greater than the material can stand, the flow will take place as indicated by the arrow in the figure. If the material flows without change of volume, as if it were incompressible, the upper part of A and its surface will be raised, the upper part of B and its surface will be lowered, the neutral level will sink and an approximation to the original conditions with complete isostatic compensation will be reestablished.

This is the general case of isostatic readjustment by the action of gravitation alone. Gravitation tends to produce a deep undertow from the regions where deposition is taking place to the regions

where erosion is in progress, in the direction opposite to that of the surface transfer of material.

Let us suppose that the isostatic compensation at a given stage in the earth's history is practically complete for a continent, that the process of erosion from the greater part of the continent and deposition around its margins is in progress, and that the process of readjustment by a deep undertow is in progress. These processes will cause changes of pressure and temperature within the earth at certain places. It is important to study the probable effect of these changes upon the condition and especially upon the density of the material involved.

At this point, in order to keep our subject in proper perspective, it is desirable to recall that the average defect in density under a continent corresponding to complete isostatic compensation is one per cent. or less, the average excess of density under an ocean only about two per cent. and the maximum defect or excess under the highest parts of the continents or under the deepest parts of the ocean are but little greater than three per cent. These are very small differences in density. Differences larger than these are frequently observed between samples supposed to be alike.

If a layer of material 1,000 feet thick is eroded from one part of the earth's surface and deposited on another part the pressures must become appreciably reduced for a considerable distance below the eroded region and increased below the region of deposition. The heterogeneous material composing the earth is continually undergoing chemical changes. The expression chemical change is here used in its widest sense, the sense in which it includes the processes of solution, crystallization and changes of state between the

solid, liquid and gaseous forms; includes the solution of gas in liquids, the solution of rock ingredients in water and their redeposition as new materials different from the original materials, and changes from an amorphous to a crystalline state, and vice versa. All these and more are concerned in the complicated processes of metamorphism. In the heterogeneous mixture at any point in the earth a great many changes are impending. A relief of pressure at any given point tends to favor such changes as are accompanied by increase of volume and reduction of density, and an increase of pressure tends to have the reverse effect. Many of these suggested chemical changes are accompanied by a change of much more than three per cent. in density. Changes of this nature in a small part of the material in any cubic mile may alter the average density as much as three per cent.

A large reduction of pressure may reasonably be expected, by favoring certain chemical changes within the earth and opposing others, to bring about gradually with the lapse of ages a decrease of two or three per cent. in the density of the material relieved of pressure.

Under a region where erosion is in progress or has recently been in progress one should expect, therefore, that the chemical changes guided by reduced pressure will gradually produce increase in volume and a raising the surface; and conversely, under a region of deposition the chemical changes guided by increased pressure will gradually produce increase of density, reduction of volume and a lowering of the surface. The surface changes will then favor more erosion and more deposition in the same regions as before. During this process the stresses due to gravitation, tending to produce an undertow and thereby an isostatic re-

adjustment, gradually increase until an undertow takes place and the isostatic condition is restored or nearly restored. In this last state the surface of the continent will still be elevated, its margins will still be low and the processes of erosion, deposition and isostatic readjustment by an undertow will still tend to continue.

Note that the processes just indicated explain the existence of defective density (light material) in the continent and to great depths below the surface, not by the supposition that the light material was there originally, but by the supposition that the processes of chemical change are such as to increase the volume and decrease the density of the material after it is in position as a part of a continent.

In studies of the earth it is frequently assumed tacitly that the material is sensibly incompressible under changes of pressure produced by the shifting of loads, by erosion and deposition. It would be as sensible as this supposition, not more absurd, to compare the material beneath an eroded surface to the contents of a vichy siphon. Upon a slight reduction in pressure, of a few pounds per square inch, the contents of a vichy siphon double their volume in a few seconds. After the reduction of pressure caused by the erosion of a layer a mile thick from the surface of the earth in a given region the material below to a depth of 76 miles probably changes its volume by one per cent. in the course of the next few ages.

Now consider the effects of the changes of temperature which would be produced by the erosion, deposition and undertow which have been indicated.

Near the surface the temperature is known to increase about 1° C. for each 100 feet increase in depth below the surface. At great depths the rate of increase is probably much smaller. Assume that it

is 1° C. for each 200 feet on an average down to the depth of compensation, 76 miles. Then if a stratum 1,000 feet thick is eroded from a region the temperature will be lowered under that region in the course of ages by 5° C. upon an average to the depth of 76 miles. Assuming that the coefficient of vertical expansion is 1 part in 60,000 per degree Centigrade, the material to the depth 76 miles will contract 1 part in 12,000 in thickness or 30 feet. On these assumptions then for every 1,000 feet eroded there is a tendency to produce by cooling and contraction 30 feet of sinking of the surface, that is, one foot of sinking by thermal contraction for each 33 feet of erosion. It is unimportant whether this ratio 1:33 is a close approximation. It is important to note that whereas the reduction of pressure caused by erosion tends to make the material expand, the lowering of temperature caused by erosion tends to make the material contract, an opposite effect.

Probably expansion by chemical change begins to occur promptly after a certain amount of erosion has occurred, since a change of pressure would probably be felt comparatively promptly even at considerable depths. On the other hand, the cooling is necessarily slow and may require ages to penetrate 76 miles. Hence following erosion in a given region the expansion due to chemical change will tend to begin first. Later, and developing much more slowly, the contraction due to the lowering of the temperature will occur. The latter may in time become as rapid or more rapid than the former, the volume may cease to increase or may even decrease, the surface may stop rising or it may even sink, and the region of erosion be changed into one of deposition.

Similarly, under a region of deposition two effects of opposite sign tend to occur.

The effect of increased pressure tends to produce chemical changes accompanied by decrease of volume and so to produce a sinking of the surface. The blanket of deposited material tends to raise the temperature in each part of the material covered, to increase the volume of this material, and thereby to raise the surface. The temperature effect may serve in time

comparatively neutral region between the two in which neither erosion nor deposition is much in excess of the other, see Fig. 2. Hence the undertow by increasing the temperature and causing a change of density may be directly effective in changing the elevation of the neutral region between two regions of deposition and erosion.

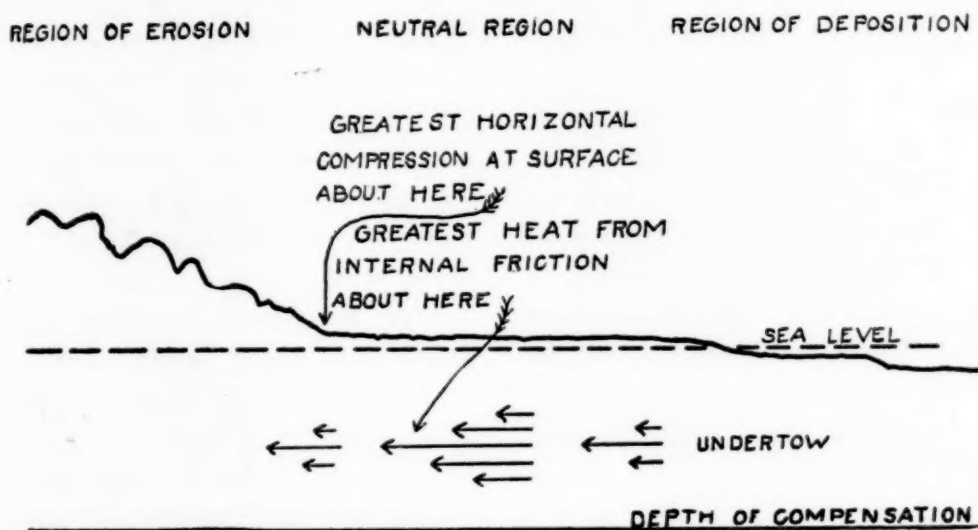


FIG. 2

to arrest the subsidence caused by increased pressure or even to raise the surface and change the region of deposition into one of erosion.

The changes of temperature just described are due directly to erosion and deposition. If as an effect of erosion and deposition an undertow is started tending to reestablish the isostatic condition, this undertow, a flow of material presumably solid, necessarily develops considerable heat by internal friction. The increase of temperature so produced tends to cause an increase of volume. It may favor new chemical changes, including changes from the solid to the liquid state, which may be accompanied by a change of volume. The undertow tends to be strongest not under the region of rapid erosion nor under the region of rapid deposition, but under the

Horizontal compressive stresses in the material near the surface above the undertow are necessarily caused by the undertow. For the undertow necessarily tends to carry the surface along with it and so pushes this surface material against that in the region of erosion, see Fig. 2. These stresses tend to produce a crumpling, crushing and bending of the surface strata accompanied by increase of elevation of the surface. The increase of elevation of the surface so produced will tend to be greatest in the neutral region or near the edge of the region of erosion, not under the region of rapid erosion nor under the region of rapid deposition.

There has been indicated a complicated set of changes of pressure, temperature and density and of movements of material beneath the surface, a set of changes which

have been started by erosion and deposition and continued by the action of gravitation tending to bring about a readjustment to isostatic conditions.

Attention is invited to the thought that during the actions indicated the pressure, temperature and relative movements in any small portion of the material are a function not simply of the facts at that place but also largely of the facts at many other places, some at considerable distances.

For example, in the neutral region between a region of erosion and one of deposition there may be movements beneath the surface (the undertow), changes of temperature in the flowing material, a crumpling of the surface material, changes of volume and changes of elevation of the surface, all of which are dependent primarily upon the facts in adjacent regions of erosion and deposition.

Let us limit our thoughts still to the cycle of changes which have been sketched roughly. Keep it in mind that the actions at any given point in the material depend on the facts at many other points. Keep it in mind also that a region of erosion in one age may, and often does, become a region of deposition in another, and that, therefore, the actions taking place at any instant in a given portion of the material are necessarily dependent upon the past as well as the present conditions. Is it not evident that even if the cycles of action which have been indicated were the only actions taking place in the outer portions of the earth, the resulting series of movements observable at the surface would be very complicated? Is it at all certain that under the influence of such actions the geological record at the earth's surface at the end of 50 to 100 million years would be appreciably less complicated than the geologic record which is actually before

us? I think that it would be fully as complicated as the actual record.

Let me illustrate, by a single example, the kind of reasoning which the considerations just stated should lead one to avoid.

It has been stated to me that mountains are sometimes eroded to a peneplain, and that thereafter the peneplain sometimes sinks. It has been suggested to me that such a case can not be reconciled with the theory of isostasy. It is said that as the material is eroded from the surface the underlying material must increase in volume to keep the isostatic compensation complete, hence that according to the theory of isostasy a peneplain may rise but never sink.

This reasoning contains several errors. In the first place, in Fig. 1, after a portion of the surface of column *A* has been carried away by erosion and the pressure at the bottom of the column thereby reduced to less than that under column *B*, the mere vertical expansion of column *A* will not reestablish equality of pressure. The equality may be reestablished only by restoring mass to column *A* by forcing material into it from some other column. This gravitation tends to do. Secondly, when gravitation, by producing an undertow, forces material into column *A* the new material may enter by processes which increase the density of column *A*. Column *A* may thus become heavier without any raising of its upper end. An eroded surface does not necessarily rise. Thirdly, a time may come when by virtue of the lowering of the temperature by erosion the material in column *A* may increase in density by thermal contraction and the surface may thus be lowered without any masses passing to other columns. If so, the isostatic condition remains unchanged, the relative pressures at the bases of *A* and *B* remaining unchanged. Such a process

may cause a region which has been eroded from mountains to a peneplain to sink thereafter. Fourthly, such reasoning as that cited ignores the history of the region before the mountains were there. That early history is essential to a full understanding of late movements. Fifthly, such reasoning entirely ignores the relations of the region considered to adjacent regions. As the evidence shows that the material concerned in isostatic readjustment is 76 miles deep, it is but sensible to estimate that the influences concerned in any one movement of isostatic readjustment extend over horizontal distances of at least 76 miles, probably over distances as great as 200 miles. Therefore, valid reasoning in regard to the peneplain in question should include a consideration of the conditions surrounding it to a distance of 200 miles, whereas in fact in the reasoning cited the surrounding conditions were entirely ignored.

How is it possible to form an estimate of the relative effectiveness of gravitation tending to produce isostatic readjustment, on the one hand, and of all other forces acting on the outer portion of the earth, on the other hand? Gravitation is the only force which continuously tends to produce isostatic readjustment. The rigidity of the material tends continuously to oppose the readjustment toward isostasy. Other forces than gravitation are equally likely to help or to oppose gravitation. Therefore, the fact that the isostatic compensation is everywhere nearly complete is a proof, first, that the material composing the outer portions of the earth has but small effective rigidity, and, second, that the forces in operation other than gravitation are relatively ineffective. If either of these propositions were untrue the present close approach to complete isostatic compensation would not exist.

Before closing let me remind you that the geodetic evidence shows that the outer portion of the earth is not a solid crust a few miles thick floating on a liquid substratum of slightly greater density.⁷

The existence of isostasy is now thoroughly established by evidence which is available in print. The time has come to speculate upon the manner in which the isostatic readjustments are produced, to look for the relations between the known condition, isostasy, and other known facts. This address is a map showing the results of a reconnaissance in this field. The reconnaissance has involved much more thought than I have been able to put into words here. Some of the statements have been made in rather a dogmatic form. That is simply because I have tried to draw a clear reconnaissance map with few strokes, not that I have forgotten that the field work has been merely reconnaissance. I feel confident, however, that in due course of time when careful surveys shall have been substituted for this reconnaissance, the main features of this reconnaissance map will be found to be correct.

In closing let me give you the reconnaissance map on a small scale with details omitted.

Readjustment toward isostatic conditions has been in progress throughout geologic time.

The differences in density involved in complete isostatic compensation are very small, usually less than one per cent., seldom more than three per cent.

With reference to such small changes of density the earth is not incompressible under the influence of stresses which are applied continuously for geologic ages.

Erosion and deposition cause changes of pressure, which in turn bring about

⁷ "The Figure of the Earth and Isostasy," pp. 163-164.

changes of chemical state in the heterogeneous material within the earth such that increase of pressure in time produces increase of density, and relief of pressure produces decrease of density.

The direct effects of erosion and deposition on temperatures in the underlying material are such as to cause changes of density opposite to those caused directly by the change of pressure and probably occurring later than those caused by changes of pressure.

Gravitation tends continuously to bring about a readjustment to isostatic conditions by producing a deep undertow from a region of deposition to a region of erosion.

This undertow by virtue of heat produced by internal friction and by virtue of surface compressive stresses in the horizontal direction tends to raise the surface of the neutral region between a region of deposition and one of erosion.

The phenomena of isostatic readjustments by gravitation are complicated. Actions involved at any one spot are a function of the facts at many other places and of the facts of earlier ages.

The material in the earth to a depth of 76 miles is weak under the action of forces applied for geologic ages. The effects of gravitation predominate over those of other forces to this depth.

JOHN F. HAYFORD

*THE MERSHON EXPEDITION TO THE
CHARITY ISLANDS, LAKE HURON*

For several years the University of Michigan Museum and the Michigan Geological and Biological Survey have been cooperating in a biological survey of the state. The survey has had a small annual appropriation for this work, and has deposited the collections in the museum, but the expeditions sent out from the latter have nearly all been made possible by gifts from persons interested in the progress of the work or in the institution.

In the summer of 1910, Hon. W. B. Mershon, Saginaw, Mich., placed in the hands of the chief field naturalist of the survey, who is also the head curator of the museum, a sum sufficient to send a small party to the Charity Islands in Saginaw Bay, for the purpose of investigating the fauna and flora.

The Charities comprise a group of three small islands situated near the mouth of Saginaw Bay. The largest of these, Charity Island proper, contains about 650 acres, Little Charity Island, the next largest, about 3 acres, and Gull Island is a small projecting reef, about a quarter of an acre in extent, that is not usually shown on the maps. The group is somewhat nearer the west coast than the east. As plotted on the Lake Huron Coast Chart No. 2, of the United States Lake Survey, the distances of the larger island (where most of the work was done) from the mainland are as follows: to Point Lookout, slightly north of west, six and seven eighths miles; from Caseville, due southeast, nine and five eighths miles; from the end of Sand Point, a little east of south, seven and three fourths miles; from Oak Point, south of east, nine miles.

The islands are of interest biologically in two ways. In the first place, as they have not been connected with the mainland since glacial times, the biota must have reached them over a stretch of water at least as broad as that which now separates them from the mainland. In the second place, they are apparently upon a migration route of many species of birds.

The men engaged to do the work and the groups to which they devoted most of their time were as follows: W. W. Newcomb (butterflies and moths), N. A. Wood (vertebrates), A. W. Andrews (beetles), Frederick Gaige (ants), C. K. Dodge (plants). The museum and survey are greatly indebted to these men, for they did the field work without other remuneration than their expenses, and are now preparing their results for publication. Acknowledgment should also be made of the assistance of the light-house board, Washington, D. C., and Commander C. B. Morgan, inspector of the eleventh light-house district,

Detroit, Mich., in granting permission to work on the islands. The assistance received in the field will be acknowledged in the several papers.

The results of the expedition will be published in various journals and in the annual reports of the Michigan Academy of Science under the common title "Results of the Mer-shon Expedition to the Charity Islands, Lake Huron." As most of the field work was done in the late summer and fall, the survey plans to continue the work in the spring and early summer of 1911.

ALEXANDER G. RUTHVEN
UNIVERSITY OF MICHIGAN MUSEUM

ARTESIAN WATERS OF ARGENTINA

THE climate of a part of Argentina is semi-arid, and the geological formations which are regarded as Quaternary and Later Tertiary are in the western and central districts of the country saline to a degree which indicates prolonged duration of aridity. The region of the pampas which covers the province of Buenos Aires and stretches northward west of the Parana does not exhibit this characteristic, it having apparently long enjoyed a more humid climate, as it does now. The foot-hills of the Andes are also well watered. But with the exception of these last-named regions, a great part of the country suffers from lack of good water. This condition may, however, be in some measure relieved by proper development of artesian supplies. Many wells have been sunk already, but without adequate geological investigation. In the pampas water is found at a general depth of 20 meters more or less, and is pumped to the surface by windmills. It may be said that the development of the livestock industry of Argentina would be impossible were it not for this supply which comes from eolian, alluvial deposits of Quaternary and Tertiary age. A different geological condition exists from the Rio Colorado southward in what may be best described as northern Patagonia. In that region there are local elevations occupying a middle position between the Atlantic and Pacific, composed of

granites and older rocks possibly of Paleozoic age, and rising to altitudes of 300 to 1,000 meters. These mountains are not represented upon any map and their distribution is not known, but they have been described by Moreno and other explorers. Upon their flanks there is an extensive formation of gray sandstone which attains a thickness of several hundred feet and is very porous. It slopes gently toward the Atlantic and pure water flows from it in outcrops near the coast. The head of water in these strata is unknown. Further south in Patagonia the central sierra is replaced by plateau country and in Comodoro Rivadavia, in latitude 46 near the coast, wells which were sunk by the government in search of water developed petroleum. There is a large area in this region in which the geologic structure and the possibilities of artesian water need to be developed. In the great plains east of the Andes there are glacial deposits which may furnish superficial supplies like those of the Dakotas, and the marine Tertiary and Mesozoic strata afford conditions not unlike those of southern California. Here as well as in the valleys among the spurs of the Andes from Patagonia to Bolivia the geological structure is complicated and the problem of artesian water is one of peculiar difficulty as well as of great interest.

Our present knowledge of these conditions rests upon reconnaissance work and the stratigraphic and paleontologic observations of the Geological Survey of Argentina. No work based upon topographic maps and systematic structure has as yet been undertaken. The problem is therefore one whose elements are as yet to be developed. The Argentine government is using every means to encourage settlement and development of the rich agricultural regions which lie in the zone of sufficient rainfall east of the Andes, and also the vast grazing district of Patagonia. In order to afford ready communication it is building railroads at great national expense and operating them. The need of pure water for locomotive use as well as for other purposes has thus been made critically evident,

and the Minister of Public Works, Senor Ramos Nexia, has adopted a plan for making surveys for the determination of artesian water conditions along the lines of national railways. He contemplates topographical and geological surveys of a character similar to those executed by the U. S. Geological Survey, from which he derived the initial suggestion. He last summer applied to the U. S. government for the services of a geologist and such assistants as he might need, and our government has responded cordially to that request. Mr. Bailey Willis has accordingly entered into a contract for the term of two years, to execute topographical and geological surveys for the specific purpose of ascertaining artesian water possibilities in those districts which the minister may designate. With him are associated Mr. Chester W. Washburne, of the U. S. Survey, Mr. J. R. Pemberton, of Stanford University, and Mr. Wellington D. Jones, of Chicago University, as geologists, and Mr. C. L. Nelson and Mr. W. B. Lewis, as topographers, and the party sails shortly for Argentina to enter upon the work. While these surveys have a specific purpose, their possibilities of usefulness in developing the natural resources and encouraging settlement in the regions surveyed will not be overlooked, and the work will be founded on these scientific studies, upon which alone practical conclusions can safely rest. Thus it is hoped that a definite contribution to knowledge in geography and geology may be made.

It is desirable to point out that the Argentine government has a geological survey which has been in existence since 1903 in its present organization and which dates back half a century as a bureau of mines. It is under the direction of Senor E. M. Hermitte, who is assisted by Messrs. Bodenbender, Keidel and Schiller, three German geologists who have done excellent stratigraphic and paleontologic work, particularly in districts of the central Argentine Andes. They have unfortunately not been supplied with maps. The established Bureau of Mines, Geology and Hydrology is under the minister of agriculture. The surveys which are about to be made are undertaken by the minister of public works. The

two operations are thus officially distinct, but it is hoped and anticipated that they may be mutually helpful.

THE ENGINEERING BUILDING OF THE UNIVERSITY OF CINCINNATI

THE new \$300,000 engineering building, and the new \$150,000 power plant of the University of Cincinnati are rapidly nearing completion. The engineering building is of reinforced concrete and stone, four stories in height, built to accommodate five hundred students, and inasmuch as the greater number will be cooperative students, the building will accommodate one thousand.

Among the main features of the building will be a large laboratory 200 × 40 feet in size. This laboratory will be surrounded by balconies, which will give a much larger floor space than is indicated by the dimensions of the room itself. In addition to this there will be a large general club room for the students taking the regular engineering courses. There will also be a large consulting library, solely for the use of the College of Engineering.

The building will be fire-proof throughout and of the best possible construction. One marked feature of the building will be the absence of a great mass of heavy machinery which is usually found in engineering colleges. The students will possess the unique advantage of having at their disposal the use of the latest and most improved machinery in all of the different manufacturing industries having plants in the city of Cincinnati. They will gain their knowledge of the different operative processes at first hand in the great manufacturing establishments, for which Cincinnati is famous. This condition has permitted the use of space which would have otherwise been occupied by machinery for extensive scientific and research laboratories.

The power plant is one of the most extensive and thoroughly equipped in the country, and has been built to meet the needs of a growing university for many years to come. It will supply heat, light and power for all of the different buildings of the university.

One marked departure from the customary arrangement of university buildings will be

found in the class-room facilities in the new engineering building. Inasmuch as the building has been erected mainly for the use of the rapidly enlarging cooperative department, it was felt by Dean Schneider that the old arrangement of class rooms was inadequate to meet the needs of the mature men who constitute a large proportion of the university student body. These men come from the various shops and large establishments of the city to the college, and in their daily experience in actual productive work, they have been confronted by many problems, not alone of theory, but of practise, and these problems have suggested to them certain very definite questions which they bring from the shops to the college for answer by their instructors. It was felt that a change in the ordinary class-room work and arrangement was needed to meet these new conditions. Each section will have a room which will be wholly its own. This room will be furnished with a table 5×10 feet, comfortable chairs, drawing tables, drawer lockers and magazine racks. Each group will have one such room, which will serve the dual purpose of club and class room.

The purpose will be to make these rooms not only places for recitation and instruction, but also sub-social centers. They will contain everything needed to satisfy the social needs of each section, and during the time when classes are actually being conducted in this room, the teacher and the men in the class room will sit around the large table and the practical and theoretical questions which the students have asked will be discussed in open session. This is a marked innovation in interior college arrangements, but the whole plan of the engineering college is being evolved to meet the special needs of the cooperative system, and any change whatsoever which promises to more satisfactorily meet the needs of a student body such as will occupy this building, will be thoroughly tried out before its adoption or final rejection.

THE INTERNATIONAL SCHOOL OF AMERICAN ARCHEOLOGY AND ETHNOLOGY

THE International School of American Archeology and Ethnology was inaugurated

in the City of Mexico on January 20. The founding patrons of the school are the government of the United States of Mexico, the government of Prussia, Columbia University and Harvard University. The University of Mexico has placed at the disposal of the school rooms in which classes may be held, and will facilitate access to libraries, museums, institutes and other scientific centers in which are pursued studies like those of the school, and will aid in the support of the school with an annual subsidy of \$6,000. Each patron will in turn appoint and pay a director of the school, and will also allot fellowships which will be sufficient to cover the expenses of board and lodging and transportation of a fellow. In accordance with the statutes the government of Prussia has appointed as director Professor Eduard Seler, director of the section of anthropology and archeology in the Royal Museum at Berlin, who has already made extensive researches in Mexico. He will hold office for one year, and will be aided by Professor Franz Boas, of Columbia, during his presence in Mexico as professor of anthropology at the National University. Two appointments to fellowships have been made, Dr. Werner Von Hürschelmann by Prussia, and Miss Isabel Ranives Castaneda by Columbia University.

All the explorations and studies of the school are to be subject to the laws of the country in which the work is undertaken, and all objects found in investigations or explorations will become the property of the national museum of the country in which the studies are carried out. In case similar specimens of the same kind of object are discovered duplicates will be given to the patrons who supply the necessary funds for the exploration. Most of the explorations will be conducted in the rich fields of Mexico, and the government of that country has already given the necessary authorization for the investigations which will soon be begun and are certain to produce interesting and valuable results.

SCIENTIFIC NOTES AND NEWS

SIR JOSEPH LARMOR, Lucasian professor of mathematics at Cambridge University and

secretary of the Royal Society, has accepted an invitation to become the unionist candidate for the vacancy in the parliamentary representation of Cambridge University.

THE Belgian Royal Academy of Sciences, Letters and Arts has awarded to Dr. L. A. Bauer, of the Carnegie Institution, the Charles Lagrange Prize for the period of 1905-08, of 1,200 francs, on account of his various researches in terrestrial magnetism.

DR. HIDEYO NOGUCHI, associate member of the Rockefeller Institute for Medical Research, received in December, 1910, from the Japanese government the honorary title of Professor (Igakuhakushi).

PROFESSOR JACQUES HADAMARD, of the Collège de France, has accepted an invitation from Columbia University to give instruction in mathematics at Columbia for a period of four to five weeks in the autumn of 1911. He will conduct one course in pure mathematics and one in mathematical physics.

It is reported that the Krupp Society has given Professor Emil Wiechert, of the University of Göttingen, 10,000 Marks to enable him to conduct experiments in aerodynamics; and also 6,000 Marks to Professor Leopold Ambronn, of the same university, for the construction of a new photographic apparatus.

THE American Philosophical Society at a recent meeting appointed a committee to memorialize congress with a view to founding a National Earthquake Laboratory at Washington. This committee consists of Dr. Charles D. Walcott, secretary of the Smithsonian Institution, chairman; Professor H. F. Reid, Johns Hopkins University; Professor William H. Hobbs, University of Michigan; Dr. R. A. F. Penrose, Philadelphia, and Professor T. C. Chamberlin, University of Chicago.

It is announced that Baron Reinach has provided the Frankfort Physical Society with the funds necessary to establish a seismological observatory on the Feldberg, in the Taunus range. Dr. F. Linke will be the director of the observatory.

PROFESSOR F. SMITH and Mr. F. A. Loew, of the University of Illinois, will this summer be associated with Professor J. E. Reighard at the Biological Station of the University of Michigan at Douglass Lake.

C. L. DE MURALT, recently appointed professor of electrical engineering at the University of Michigan, becomes editor of the *Railway Electrical Engineer*. This journal is the official organ of the Association of Railway Electrical Engineers.

DR. J. J. DAVIS, of Racine, Wis., who has devoted a large amount of time to the study and collection of parasitic fungus flora of Wisconsin, has been appointed curator of the herbarium of the University of Wisconsin. On their transfer to the new biological building, the botanical collections will be provided with new and better quarters for work, and a complete reorganization of the museum is planned.

FOURTEEN Harvard professors will be absent during the second half of the current academic year. They include: Professors C. L. Jackson, of the chemistry department; Hugo Münsterberg, of the philosophy department, who is serving as exchange professor at the University of Berlin; J. L. Love, of the mathematics department; C. L. Bouton, of the mathematics department; W. Z. Ripley, of the department of economics; R. B. Dixon, of the division of anthropology, and C. R. Sanger, of the chemistry department.

THE following officers were elected at the recent annual meeting of the Royal Meteorological Society: *President*, Dr. H. N. Dickson; *vice-presidents*, F. Druce, H. Mellish, R. G. K. Lemfert, Colonel H. E. Rawson, C.B.; *treasurer*, Dr. C. Theodore Williams; *secretaries*, F. C. Bayard, Commander W. F. Caborne, C.B.; *foreign secretary*, Dr. R. H. Scott, F.R.S.

THE Nashville section of the American Chemical Society held its organization meeting at Furman Hall, Vanderbilt University, on January 25. After the adoption of a constitution the following officers were elected: *chairman*, W. L. Dudley; *vice-chairman*, J. I.

D. Hinds; *councillor*, R. W. Balcom; *secretary and treasurer*, L. J. Desha. Dr. W. L. Dudley gave an informal talk on the "Action of Wireless Waves on Rarefied Gases." The regular meetings of the section will be held on the third Friday of each month.

DR. H. W. WILEY, chief chemist of the Department of Agriculture, Washington, D. C., delivered an address at Syracuse University on February 1 upon "The Services of Chemistry to the Public Welfare." The meeting was held in the Bowne Hall of chemistry under the joint auspices of the Syracuse Chapter of Sigma Xi and the Syracuse Section of the American Chemical Society.

PROFESSOR C. K. LEITH, of the University of Wisconsin, gave a lecture before the advanced students in geology at Northwestern University on January 26. His subject was a comparison of the origins of the iron ores of the Lake Superior region, of Cuba and of Brazil.

A LECTURE on electric oscillations and their application to wireless telephony was delivered before the chapter of Sigma Xi at Purdue University, LaFayette, Ind., January 28, by Professor C. M. Smith, of the department of physics of that university. Professor Smith explained the theory of wireless telegraphy and telephony and pointed out the entirely different conditions necessary for wireless telephony as compared with wireless telegraphy. The lecture was illustrated by a large number of experiments showing the analogy between electric and sound waves and was concluded with a demonstration of the singing arc lamp which reproduced very clearly a band selection through the agency of a phonograph and microphone located in a distant room.

AT a meeting of the Royal Geographical Society on January 16 Dr. Johan Hjort gave a detailed account of the Michael Sars North Atlantic deep-sea expedition of 1910, which he, with Professor H. H. Grau, Dr. Helland-Hansen, Mr. E. Koefoed and Captain Thor Iversen, undertook at the suggestion and at the expense of Sir John Murray, who himself accompanied them.

DR. HANS GRETHER, of Karlsruhe, Germany, has been appointed a special lecturer in McGill University and is giving in the graduate school, during the present session, a course of advanced instruction in the "Computation of secondary stresses in bridge trusses and other framed structures." The following gentlemen will act as special lecturers in the course on economic geology at McGill University during the present session: R. W. Brock, Esq., M.A., director of the Geological Survey of Canada; Dr. J. D. Irving, professor of economic geology, Yale University, and O. E. LeRoy, Esq., M.Sc., of the Geological Survey of Canada.

A STATE Biological Survey has been organized at the University of Colorado, the work being in the hands of a committee consisting of Professors F. Ramaley, T. D. A. Cockerell and J. Henderson. The work of such a survey has been carried on for a number of years past, but until now there has been no definite organization. The work includes fossil as well as living species of plants and animals.

THE British Treasury has, on the recommendation of the development commissioners, made a grant to the Board of Agriculture and Fisheries from the development fund of £40,000 for the ensuing year for the encouragement of light horse-breeding in Great Britain.

THE third semi-annual meeting of the American Institute of Chemical Engineers will be held at Chicago, Ill., June 21 to 24. Arrangements will be made to visit a number of the large technical plants in the vicinity. The committee on chemical engineering education and standardization of boiler tests will have important reports to present. The program of papers will be announced later.

THE first Universal Congress of Races will be held in London from July 26 to 29, 1911, to discuss the general relations between western and eastern peoples.

A COURSE of nine public lectures on problems of psychology have been given at Columbia University, as follows:

January 31—"Traits of Dreams," Professor C. E. Seashore, University of Iowa.

February 1—"Social Psychology," Professor Charles H. Judd, University of Chicago.

February 2—"Memory and Imagination," Professor E. B. Titchener, Cornell University.

February 3—"Frailties of Imageless Thought," Professor J. R. Angell, University of Chicago.

February 4—"The Standpoint and Scope of Social Psychology," Professor Mary Whiton Calkins, Wellesley College.

February 6—"The Psychology of Dream States," Professor Joseph Jastrow, University of Wisconsin.

February 7—"The Rôle of the Type in Simple Mental Processes," Professor W. B. Pillsbury, University of Michigan.

February 8—"The Ontological Problem of Psychology," Professor George T. Ladd, Yale University.

February 9—"Some Psychological Topics Emphasized by Pragmatism," Professor Josiah Royce, Harvard University.

THE new Oceanographic Institute, which Prince Albert of Monaco has erected on a part of the site of the old convent of the Dames de Saint-Michel in the Rue Saint-Jacques, was formally inaugurated on January 23. We learn from the *London Times* that the opening ceremony was performed by Prince Albert in the presence of President Fallières, M. Emile Loubet, members of the government and the principal dignitaries of the university and city of Paris. In his inaugural address Prince Albert explained the motives which had prompted the foundation of the new Institute in Paris and the purpose which he had designed it to fulfil as the complement of the Oceanographic Museum that he had founded at Monaco last year. The minister of public instruction, on behalf of the government, the president of the Academy of Sciences, on behalf of the French Institute, and the vice-rector of the university each returned thanks to Prince Albert for his munificent foundation. The new institute is at once French and international in character. This latter aspect of the foundation is marked by the presence on the committee of Sir John Murray, Professor Buchanan, Professor von Drygalski, Dr. Nansen and other foreign men of science. In addition there is an administrative council composed of French men of

science. The institute is designed to work in intimate cooperation with the museum at Monaco, where laboratory work will be conducted, while in Paris lectures on the principles of oceanography will be delivered.

THE following resolutions favoring a federal grant to elementary and secondary education were passed unanimously by the house of representatives of the Illinois legislature on January 18:

WHEREAS, The legislature of Illinois by the joint resolution of February 8, 1853, was the first among American legislatures to petition the congress of the United States to make a grant of public land for each state in the union for the liberal endowment of a system of industrial universities, one in each state, to promote the more liberal and practical education of our industrial classes and their teachers; and,

WHEREAS, The congress not only made a liberal grant of land in the year 1862 for this purpose but has also followed up this policy once begun by still more liberal appropriations for the support of higher education in agriculture and the mechanic arts, resulting in the great chain of colleges for agriculture and the mechanic arts to be found in every state and territory in the union; and,

WHEREAS, The time has now come for the adoption of a similar policy in the field of elementary and secondary education. therefore, be it

Resolved, by the house of representatives of the state of Illinois, the senate concurring herein, That the congress of the United States be respectfully petitioned to appropriate annually to each state and territory in the union a sum equal to one dollar per head of the population of said state or territory as ascertained by the last census, for the purpose of establishing, maintaining and extending in the elementary and secondary schools of said states and territories, while not excluding other elementary and secondary subjects, such practical, industrial and vocational training, including agriculture, the mechanic arts, domestic science, manual training, commercial subjects and such instruction in other similar subjects of a practical nature as the interests of the community may seem to demand; and

Resolved further, That our senators in congress be instructed and our representatives be requested to use their best exertions to procure the passage of a law of congress donating said sum to each

state and territory in the Union for said purpose; and

Resolved further, That the governor of this state is hereby requested to forward a copy of the foregoing resolutions to our senators and representatives in congress and to the executives and legislatures of each of the other states and territories, inviting them to cooperate with us in this meritorious enterprise.

ACCORDING to a statement by Mr. Ray Priestley published in the papers before the departure of the *Terra Nova* for the Antarctic, an important geological discovery was made during Sir Ernest Shackleton's expedition. Mr. Priestley, who is now engaged with Captain Scott's Antarctic expedition, and who had for some months been collaborating with Professor David at Sydney in arranging a memoir of the geological work of Sir Ernest Shackleton's expedition, states that he discovered a small piece of rock on the Beardmore Glacier which now upon full examination proves to belong to the Cambrian limestones. It appears that a similar formation has in recent years been discovered in South Australia by Mr. Griffith Taylor, who is also a member of Captain Scott's scientific staff. The fossils found both in the latter and in the Antarctic specimens are identical, and the inference is that at a not very distant past the Antarctic was united to the continent of Australia. The fossils referred to are the immediate predecessors of corals and sponges.

UNIVERSITY AND EDUCATIONAL NEWS

A GIFT of \$300,000 by Mrs. Russell Sage to Cornell University is announced. The money is to be used for a new dormitory for women students, to be known as the "Prudence Risley Hall" in honor of Mrs. Sage's mother.

THE old Philadelphia Dental College at Eleventh and Clinton Streets, which was purchased several months ago by Jefferson Medical College for \$45,000, after remodeling will become the Daniel Baugh Institute of Anatomy.

AN increase in the income and in the building fund of the University of Wisconsin on the basis of a growth of 23 per cent. in the number of students in the last two years and

of the constantly growing demand on the part of the people of the state for expert assistance from the university, is provided for in a bill introduced in the state legislature. It provides for changing the present two sevenths of a mill tax on the assessed valuation of all property of the state for maintaining the university to three eighths of a mill. This will increase the general university income approximately from \$750,000 a year to \$1,000,000 a year. For new academic buildings and permanent improvements the proposed legislation appropriates \$300,000 a year, of which \$50,000 annually is set aside for the purchase of books, furniture, apparatus and equipment. The remaining \$250,000 a year is to be used for the construction of academic buildings, in the order of their greatest need and for the enlargement and repair of present buildings. For the construction and equipment of women's and men's dormitories the university bill provides for an annual appropriation for four years of \$250,000. Out of this \$1,000,000 a woman's dormitory is to be built first, then a commons and union for men, and finally dormitories for men. The university extension would have \$100,000 next year and \$125,000 the following year. This is an increase of \$50,000 a year over the present appropriation. For agricultural extension, including traveling schools of agriculture and lectures and demonstrations throughout the state, \$40,000 is provided, an increase of \$10,000 over the present amount.

THE regents of the University of Michigan have applied to the legislature for a grant of \$250,000 for a science building. The need for more adequate accommodations for the natural sciences has been felt for a number of years, and was the subject of a memorial to the regents, by the departments of botany, zoology, geology, mineralogy and forestry, in 1907. The congestion of that time has steadily become worse with the increase of students, and only slight possibility of expansion with present buildings. In 1908 the faculty of the entire literary department unanimously adopted a resolution to the effect "that in the opinion of this faculty, the greatest present

need in the material equipment of the department of literature, science and the arts is a new building for the natural sciences."

DISCUSSION AND CORRESPONDENCE

UNIVERSITY FELLOWSHIPS

AFTER reading the address of Dr. Jordan recently published in *SCIENCE*, I desire to enter a protest against some of his statements. I have been for many years in touch with graduate students, and have been moved with a keen desire to induce them to enter the teaching profession. I thus know of the difficulties they face and why many of them fail to fulfill the hopes I had in them. I agree with Dr. Jordan that we are not producing the scholars we should, but in the diagnosis of evils we differ. In his address he comes back again and again to the fellowship system and talks touchingly of the starving doctor of philosophy. In my opinion, the starving doctor is a figment of the imagination. It is the rapidity of promotion, not the lack of it, that ruins promising investigators.

The University of Pennsylvania has had a system of fellowship long enough to make its effects apparent. Twenty-four Harrison fellowships have been granted annually for fifteen years. Few of the fellows were, however, graduates of Pennsylvania. The effect of this will be apparent when it is recognized that from the fellows instructors are chosen and from them in turn the professors come. Practically all the instructors and younger professors are graduates of other colleges. Our young men are a cosmopolitan body representing nearly every college and university in the country. The result has been a transformation of the university in a deeper and more vital way than any other of our important changes. Besides these fellows who have become teachers there has been another group coming from the smaller colleges where they were instructors and who have returned to them after a couple years' study here. These two groups account for nearly all our former fellows.

The following table gives the present occupation of all who have been fellows:

Professors and instructors in universities and colleges	107
Normal and secondary teachers	31
Literary work	5
Business and business experts	8
Government experts	6
Chemical experts	4
Social work	7
Ministers	5
Students	10
Deceased	8
Unknown	2
Total	193

This does not look like starvation. If we had double the number of fellowships we could double the service we render to our own and to sister institutions without overstocking the market. The fact is a good instructor pays his way everywhere. It is the professor that needs endowment.

Where then is the trouble if it is not in this quarter? Here again I shall turn to my own experience, which, however, I believe is that of many others. I find among the fellows a man of promise. He is made assistant at \$800 a year, then instructor at \$1,000, which is steadily increased until at thirty he is earning \$1,500. Now comes the test under which so many break down. He has published a thesis, written several articles, and has become a proficient teacher. This makes him a man of the kind that college presidents want and friends praise. It is one of the peculiarities of college presidents that they want "men of promise," they never seek for "men of deeds." This young man should settle down on his \$1,500 a year and do work that would advance his science. But the attractions of salary and the flattery of friends are too much for him. He drops his original work for more pay and finds that hastily constructed books help him along more rapidly than original work. This is the last of him so far as science is concerned. Let me give a couple examples. A young instructor was pushed along until he had the \$1,500 a year. He then received an offer of \$2,500 from another college. I talked to him in this way: "You are familiar with the courses you give and your hours are reasonable. Now is the

time to use your leisure to do original work. The next five years will settle what your scientific standing will be. See that you make good. I can not get for you an increase of salary, but I can get for you every facility for good work." I thought I had my man, but he came to me a couple days later saying he had decided to go, as his wife thought she could not live on \$1,500 a year. As a second case I take that of a young man in another institution in whose work I became interested. When a book of his appeared, I wrote him that I was sorry he printed it. It did not fulfil the expectations I had of him, and I believed no man could afford to be the author of a useless book. He replied that he was glad others were not of my opinion and sent with the letter several laudatory clippings from papers and eulogistic letters from professors with reputation. This, of course, was a great victory and in a way I admit he was right; for the book brought him a call to a leading university. But a book of promise is yet to come. This, not starvation, is the road to ruin. Young men are not spoiled as fellows, but as assistant professors. A call means new responsibilities, the breaking up of old habits and a loss of self-discipline. The new president calls him a second Agassiz; the university press bureau spreads laudatory notices of him in the local press and the alumni take a hand in extension of the fame of the new genius.

Dr. Jordan tells us that he has been working for others the greater part of his life and that he is disappointed in the results. But for whom has he been working—for fellows or for assistant professors? There are no fellowships at Stanford University. If he would go over his cases, he would, in my opinion, find that he, like other college presidents, has been dragging into the lime light young men that it would have been better to have let alone. Each university should build up its faculty quietly from its fellows instead of running press bureaus to laud immature men. Scholars are not born, they are made by their environment.

No one is worth keeping who will not halt

long enough on \$1,500 a year to do good work. The assistant professorship is an unearned entrance to the halls of learning. If faculties would agree that no one should have the title of professor until it was fully earned, the increase of true learning would be possible. Scholarship is made by hard work, and comes only with gray hairs. If a man is wanted from another university take its best. Young men should be left alone until they are fully developed before transplanting them.

S. N. PATTEN

UNIVERSITY OF PENNSYLVANIA

TO THE EDITOR OF SCIENCE: In response to a recent friendly note from Dr. Edmund B. Wilson let me say: No money could be better spent than that used for the fellowship which enabled Wilson to walk and work with Brooks and Martin and Remsen. But too much such money is used to hire mediocrity to make diagrams for pedantry.

Our scholars must in some degree be descended from scholars. Relatively few of our teachers have the personality which befits the leader in an intellectual school. The scholar should be free to seek such leadership, and our present fellowship machinery tends, on the whole, to confuse rather than to help.

DAVID STARR JORDAN

THE ARIZONA PASSENGER PIGEONS

THE passenger pigeon is now generally believed to be extinct in a wild state, and of those formerly living in confinement only a single survivor, in the Zoological Garden at Cincinnati, remains. Under these circumstances reminiscences of its past history naturally find place in ornithological and other journals, based on the recollections of observers still living or gleaned from the published narratives of early travelers and explorers of the birds' former range, some fifty pages of such matter having appeared in the last two numbers of *The Auk* alone. Among recent contributions to passenger pigeon lore is Dr. McGee's "Notes on the Passenger Pigeon," published in a recent number of SCIENCE.¹

¹ Vol. XXXII., pp. 958-964, December 30, 1910.

Dr. McGee's paper is divided into two parts, the first giving his recollections of the great abundance and habits of the passenger pigeon as seen by him in eastern Iowa nearly half a century ago, the other an account of birds supposed to be passenger pigeons seen in arid southwestern Arizona as recently as 1905. His account of the abundance of these birds during the spring migration in eastern Iowa "in the sixties and early seventies" of the last century is a fact of great interest and is in accord with what is known to have occurred in the eighteenth and the early part of the nineteenth centuries in states further to the eastward. But the habits of the Iowa pigeons, as here detailed, during the breeding season and until and during the fall migration, are wonderfully suggestive of the habits of the mourning dove, and depart considerably from the habits of the passenger pigeon as observed and repeatedly recorded at points further eastward; as, *e. g.*, their laying two white eggs, living in family groups during and after the breeding season, and in this manner taking their departure southward at the approach of winter.

The second or Arizona part of the paper is entirely contrary to our previous knowledge of the distribution of the species, and especially contrary to everything known of its breeding area. It has not heretofore been recorded as occurring west of the eastern border of the plains, while its known breeding area was the transition zone of the east. To enable a bird with these geographical and physiological restrictions to pass the hot season and rear its young in the subtropical Lower Sonoran zone of southwestern Arizona implies a most wonderful range of adaptability, and one quite unparalleled in our present knowledge of bird life. Not that some species of birds, the mourning dove among others, do not have breeding ranges that cover the greater part of North America, and seem equally at home, even in the breeding season, in regions as unlike as the humid wooded districts of the eastern states and the arid southwest; but there are others, like the passenger pigeon, which are restricted to a particular

type of country, especially during the breeding season. From their known distribution, habits and food requirements, one would almost as soon expect to find a colony of ptarmigan, an alpine or semi-arctic bird, in Florida as passenger pigeons in the arid, almost forestless Lower Sonoran zone of southwestern Arizona. The passenger pigeon occupied the wooded districts of eastern North America, breeding from eastern Kansas, northern Mississippi, Tennessee, Pennsylvania and New York northward to western Mackenzie, central Keewatin, central Quebec and Nova Scotia, and usually in large colonies, it being at all times preeminently gregarious. If formerly found west of the great plains, it is very strange that none of the scores of ornithologists who have either lived for many years in the general region of Arizona and New Mexico or have during the last two or three decades thoroughly explored it in all parts, down to and along the Mexican border, have ever collected a specimen anywhere in this whole area that has been identified by a competent ornithologist as a passenger pigeon. Again, Dr. McGee's account of the nesting and other habits of the birds he took to be passenger pigeons at Tinajas Altas in Arizona are not incompatible with those of the mourning dove, its little brother, known to be of common occurrence in just the situations described. Furthermore, the bird there known as the "Sonora pigeon," and referred to by Dr. McGee as "seen singly and by twos and threes, either distant or in flight," and "noted as resembling the passenger pigeon," is the white-winged pigeon (*Melopelia asiatica*, formerly *M. leucoptera*). "The Sonora pigeon (at least the bird observed at Tinajas Altas) differs so widely as to be readily distinguishable from the mourning dove," and of course also from the passenger pigeon. It is extremely to be regretted that "unexpectedly hasty abandonment of the camp unfortunately prevented preservation of skins of the birds," for while no one will doubt the author's sincerity and conscientiousness in placing on record his recollections of these birds, it is certain that ornithologists will desire more

substantial evidence of so improbable an occurrence as the breeding of the passenger pigeon in arid southwestern Arizona before they will be willing to accept these observations as a part of the history of a now practically extinct species. If specimens of the birds in question had been obtained and identified by competent authority, it would doubtless have saved burdening the literature of the wild pigeon with another questionable record, and one that may prove extremely difficult to eliminate.

J. A. ALLEN

ON THE TRANSFERENCE OF NAMES IN ZOOLOGY

As the preparation of an official list of *nomina conservanda* is now under consideration by the International Commission on Zoological Nomenclature it may not be out of place to call attention to a point that seems to me of prime importance in this connection, although it has received little notice from recent writers on nomenclatorial reform.

It is simply this—while the rejection and replacement of familiar names for well-known animals is, of course, an inconvenience to zoologists, it is a trivial matter in comparison with the grave possibility of confusion that arises when the names are used in an altered sense. In the former case we merely multiply synonyms, and, unfortunately, they are so numerous already that a few more hardly matter; in the latter case there is a real and serious danger of ambiguity. Thus, at present, a writer who mentions *Trichechus* may be referring either to the Walrus or the Manatee, *Simia* may mean either the Orang or the Chimpanzee, *Cynocephalus* may be either a "flying Lemur" or a Baboon, and so on through all the great groups of the animal kingdom till we come to *Holothuria* which may refer either to a sea-cucumber or to a Portuguese man-of-war. Cases like these seem to me to be on an entirely different plane, as regards practical importance, from those in which an old name is simply rejected; even if the shore-crab is to be called *Carcinides* for the future we have only the additional burden of remembering that it was once called *Carcinus*.

A striking (if somewhat exceptional) instance of the pitfalls that are in preparation for future students is found in the section on Crustacea in Bronn's *Thierreich* (Bd. V., Abth. ii.). On p. 1056 there is an allusion to "*Astacus*" and on the following page to "*Astacus* (= *Homarus*)." In the bound volume (unless the part-wrappers have been kept in place) there is nothing to show that a change of authorship intervened between these two pages and that, while the second "*Astacus*" refers to the lobster, the first indicates the crayfish.

If the International Commission could be persuaded to consider first those names that are threatened with *transference*, before proceeding to deal with those that are merely in danger of *replacement*, they would, I believe, secure the support and cooperation of many zoologists who have doubts as to the practicability of the schemes lately put forward.

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SCIENTIFIC BOOKS

African Mimetic Butterflies. By H. ELTRINGHAM. Oxford, Clarendon Press. 1910.

The remarkable resemblances often observed between insects of different genera, families or even orders, have long attracted the attention of naturalists. In some, probably many, cases the explanation may be found in parallel variation, or similar conditions of life. Such explanations do not go far into the heart of the matter, but they are satisfying to those who like to give a "reason" for everything. Bates, who was so familiar with the insect-fauna of the Amazons, hit upon a more special "reason" for resemblances observed by him. This was, in short, that certain species which were edible simulated others which were distasteful and so gained protection. The subject was taken up by Wallace and other naturalists, and soon a large body of evidence was available, especially in relation to butterflies. It was proved to be a fact that certain

forms were disliked by birds and other natural enemies; it was shown that young birds did not instinctively reject these insects, but that after having tried and found them nauseous, they avoided them subsequently. It was then not difficult to see that if an edible species came to sufficiently resemble an inedible one it would be often taken for it and so escape. At this point it was observed that sometimes there were two or more butterflies very much alike, but all inedible. Fritz Müller pointed out that there would be gain in this, since the experience obtained in tasting one might suffice to cause a bird to reject all subsequently; whereas if they were all alike, each would separately have to pay its tribute to inexperience. Thus there were recognized two sorts of "mimicry," called the *Batesian* and *Müllerian*, respectively.

Examples of these phenomena have especially been observed in the tropics, where substantially the same conditions have existed for long ages, and living things have had time to develop some very nice adjustments and interrelations. In the volume just issued, Mr. Eltringham has taken up the mimetic butterflies of Africa, and has covered the ground so well that any reader may gain a good knowledge of the main facts without access to a collection of African specimens. There are given no less than 176 excellent colored figures illustrating the different species, varieties and sexes, while in the text each one is discussed at some length. There are also sufficient bibliographical references. In addition to the matter indicated by the title, there is a good general discussion of the whole subject of mimicry, and a summary of the evidence relating to natural enemies. Thus Mr. Eltringham's book may well serve as a guide to those taking up the subject and will be found useful in biological departments of universities, where "mimicry" is discussed along with other biological theories.

A special chapter is devoted to objections to the theory of mimicry, but those wishing to see the strongest adverse arguments should consult Professor R. C. Punnett's paper on mimicry in Ceylon butterflies, recently (Sep-

tember, 1910) published in *Spolia Zeylanica*, Vol. VII. Professor Punnett spent two months in Ceylon investigating some well-known cases, and came to the conclusion (or fortified a conclusion previously reached?) that the phenomena should be explained in quite another manner. Professor Punnett, like Mr. Eltringham, gives us admirable colored figures, and his discussion is most interesting. Some of the points brought forward are the following: (1) In Ceylon birds seem not to be serious enemies of butterflies. The chief enemies are apparently lizards and Asilid flies, and these appear to lack discrimination. (Experiments with a lizard were made.) (2) In various specified cases, the "model" and "mimic" do not occupy the same area to any extent, or the "model" is scarce when the "mimic" is common, a condition irreconcilable with the Batesian theory. (3) The resemblance is often imperfect, and when the flight of the insect is different it seems unlikely that they should be confused.

Against evidence of this sort may be placed the abundant data of Wallace, Marshall, Trimen and others, who have spent long years in the tropics, instead of a short two months. It may be reasonably urged, however, that if in only a few cases it can be demonstrated that "mimicry" has a meaning quite different from that assigned by Bates and Müller, serious suspicion is thrown on the whole theory or group of theories. Professor Punnett, long associated with Professor Bateson, is of course well known as an ardent Mendelian, and it is not a surprise to find at the end of his paper a Mendelian interpretation of mimicry. As he states, breeding experiments are urgently required, but judging from the classic experiments of Doncaster on *Abraxas*, he formulates an hypothesis to account for the polymorphism of mimetic *Papilio* in Ceylon. We can not take the space here to copy his tables, but the results he gets appear to coincide with the facts. Incidentally he cites the case of *Colias edusa*, in which the pale *helice* form of female, crossed with a normal male (necessarily so, as *helice* occurs only in the female) gave females of

both *edusa* and *helice* types. He remarks that the fact that *edusa* ♀ can come from *helice* appears to disagree with his hypothetical scheme, but he adds that the typical female differs from the male, and suggests that there may be or have been a possible "a type" of female resembling the male (such a type is well known in the *Papilio* under discussion). It is interesting to be able to state that the hypothetical "a type" of female *C. edusa* is actually known, and may be found mentioned in *Entomologist*, 1889, p. 26.

I can not help thinking it probable that whether or not the precise Mendelian hypothesis offered by Professor Punnett is justified by subsequent research, the facts will be found to be very much as he has postulated. I do not think, however, that the theory of mimicry is thereby contradicted. According to the old view that all organisms are everywhere varying (aside from non-inherited environmental effects), and that natural selection is necessarily in continuous operation to keep them constant or to modify them as needed, it must be confessed that some of the observed facts are hard to interpret. According to the newer view that "original variations" happen at relatively rare intervals, and that a stable type once produced may continue indefinitely if not discriminated against, the matter assumes a very different aspect. Consider the great antiquity of insect genera, as shown by fossil remains; consider the kaleidoscopic changes in insect-type producing innumerable species often without material advance in the general type; with all this time and change there must have been produced many pairs of more or less unrelated species resembling one another. When this resemblance has been advantageous it has been preserved, while other forms have died out; and hence to-day the proportion of such cases is vastly greater than we could expect from chance coincidence. It is not necessary that everywhere and at all times mimicry should be functional; the evidence seems to show that it generally is, and that is sufficient. Indeed, if a type has been preserved because of its ability to "throw" mimetic forms, it is likely enough to continue

to do this, even in places where this is unnecessary.

Those who are confronted by the vast array of insect species rarely think of the unseen gaps in the ranks. These may perhaps be best appreciated by considering the fauna of the Hawaiian Islands, as elucidated by Sharp, Perkins and Walsingham. Here we have large genera with multitudes of allied species, no doubt the result of the immigration in ancient times of single types of a few groups. Comparatively free from the stress of competition these Hawaiian groups have, as it were, nearly their full membership; on continental areas only remnants usually remain.

Thus I think that the newer work on heredity, read aright, only strengthens the theory of mimicry, by relieving it of a load it was ill-fitted to carry. I do not see any other plausible way of accounting for the facts, unless it is by supposing that similar environments give rise to similar modifications of the germ-plasm. This idea loses support when we remember the cases (*e. g.*, in butterflies and bees) in which the same superficial appearance is due to entirely different structures.

It may still be debated whether natural selection has had much to do with the production of mimetic forms, in the sense of bringing about the *accumulation* of favorable variations. For my own part, I can not doubt that this cumulative effect of selection is real, and is a necessary cause of the more striking and complex instances of mimetic resemblance. The rarity of original variations, while great enough to relieve selection from the necessity of acting continuously on all characters, is doubtless not so great as to prevent it from bringing about many striking cumulative results, in the manner postulated by Darwinians.

I have wandered too far away from Mr. Eltingham's book, but I must return to it to mention his remarkable experiments with the larva of a moth, *Odontoptera bidentata*. Larvæ fed on ivy were offered to a lizard, and found extremely distasteful. They were, although of cryptic coloration, nearly always rejected by the reptile. Several larvæ were then transferred to apple, and after feeding on this

plant for a few days were again offered to the lizard, which ate them readily. Thus it is shown that a mere change of food plant may be of great importance in relation to destruction by natural enemies; furthermore, that some distasteful larvæ do not possess "warning" coloration, and again, that these cryptically colored larvæ were not recognized, after a few days, as objectionable. It would be interesting to repeat the experiment, having, if possible, ornamented the larvæ in some way so that they would be more easily recognized.

T. D. A. COCKERELL

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Sewage Disposal. By LEONARD P. KINNICUTT, Director of the Department of Chemistry, Worcester Polytechnic Institute; C.-E. A. WINSLOW, Assistant Professor of Biology and Biologist in charge of the Sewage Experiment Station of the Massachusetts Institute of Technology, and R. WINTHROP PRATT, Chief Engineer of the Ohio State Board of Health, late director of Sanitary Engineering of the Republic of Cuba. New York, John Wiley & Sons. Price, \$3.

This octavo book of 421 pages consists of a well-blended recital of American and European, especially English, experiences which have established the principal features now recognized in the science and art of sewage disposal. Almost without exception it is free of views that are either radical or so old-fashioned as to be regarded as superseded.

The joint authorship of this book has much to commend it and it will be noted that it includes in Professor Kinnicutt one of the foremost sanitary chemists in America, and one who has been fortunate enough to make numerous inspection trips to sanitary works in Europe, during the past thirty years. Professor Winslow, formerly of the Institute of Technology, in Boston, now of the College of the City of New York, has had unusual opportunities of studying the biology of this subject, particularly in connection with extensive experiments made at Boston. The practical side, from an engineering standpoint,

has occupied the attention of Mr. Pratt for many years, first in Massachusetts and later in Ohio, with a valuable experience in Cuba.

The chemical and biological aspects of the book are more comprehensive and detailed than those of an engineering nature. Probably this is wise in a book of this size on a subject of such a wide scope as this one and which is undergoing such rapid changes in some of the more important aspects of engineering practise. Numerous references are given to details of results obtained from the findings of the Royal Commission on Sewage Disposal of Great Britain, as well as the results of tests and practical operations in America and abroad, especially in England. References are rather meager as to German investigations and experiences. To some extent the same is true of the results of current practise in the design and operation of disposal works in the United States other than in Massachusetts and Ohio.

After an interestingly stated introduction as to the sanitary demand for sewerage and sewage disposal, the book is divided into thirteen chapters, of which brief mention may be made to advantage as follows:

Chapter 1, pp. 1-20, deals with the composition of sewage in the terms of the analyst. Chapter 2, pp. 21-44, outlines the disposal of sewage by dilution. Chapter 3 gives many details as to the screening and straining of sewage, pp. 45-67.

The preliminary treatment of sewage by sedimentation, chemical precipitation and septic process occupies Chapters 4, 5 and 6, pp. 68-166. These chapters are unusually well-written, although they do not bring fully up to date very recent developments with the so-called "Imhoff" tanks, which have shown themselves to be a marked step in advance during the past year or two in practical operations in western Germany.

The expensive, bothersome and frequently unsuccessfully solved question of the disposal of sewage sludge is well outlined on pages 167 to 192.

Chapters 8 to 11, inclusive, on pages 193-274, contain a well-balanced statement of ex-

periences in this country and abroad as regards filtration in intermittent sand filters, contact beds and trickling filters.

The remaining chapters, pp. 375-409, contain first a full statement of the recent work done in this country in the sterilization or disinfection of sewage, with data as to the efficiency and cost, while the book is concluded with a brief summary of the main features of sewage analysis with particular reference to those tests of most benefit in practical operations.

The book is very attractively written and is well indexed. There are 113 figures illustrative of the various distinctive features of the principal processes. The more one studies the book the more apparent it is that there has been a vast amount of study given to the compilation of a wide fund of information so as to embody it compactly for convenient reference. The book is free to an unusual extent of statements to which exceptions will be taken by experienced sanitarians. The principal points on which there would be differences in opinion are in reference to the residual quantity of dissolved oxygen which would be found in a stream into which sewage has been discharged, and the disparaging reference to automatic controlling devices for the operation of contact beds.

Taking the book as a whole, it may be safely said that it will be of much assistance in the class-room in teaching this subject to students and especially to the public hygienist desiring to get a general insight into the subject in its broader phases, with ample opportunity to ascertain where the various results with different styles of plants have accomplished definitely recorded results.

GEO. W. FULLER

The Practise and Theory of the Injector. By STRICKLAND L. KNEASS. Third edition, revised and enlarged. New York and London, John Wiley and Sons. 175 pages, 53 illustrations and diagrams.

This book possesses the great merit of having been written by one who is a master of his subject. It is no ordinary compilation; it

is the reflection of a life work. Its author for more than a quarter of a century has given serious attention to the problem of perfecting the injector. He has made it a part of his business to study the fundamental principles underlying its action, to conduct experiments which would supply data with which to embellish the theory, and to contribute to the working out of actual designs which from time to time have become the standards of a great manufacturing company; yet such is his modesty that nothing which is printed suggests his personal activity in the development of the instruments he describes. The book presents in logical order the fascinating story of the development of the steam injector, an instrument which serves to feed water to a steam boiler through the action of a jet of steam drawn from the boiler which is fed. In the language of the book, "its mode of action, extraordinary in appearance, contrary to that which we are in the habit of seeing or supposing, is explained by the simplest laws of mechanics and has been foreseen and calculated in advance." The book is interesting throughout because its story is well told. It deals with a subject which can not be freed from mathematical theory, in a manner which is sufficiently complete to satisfy the most fastidious lover of equations, and yet the work is so admirably arranged that no one who is interested in the subject is likely to find difficulty in reading it.

The introductory chapter on the early history of the development of the injector, from which the lines quoted above were taken, is chiefly a story of the achievements of Henri Jacques Giffard, who as early as 1850 had succeeded in developing the principles underlying the design of the present-day instrument. The injector, as a device for feeding boilers, was introduced into England in 1859, and into this country by William Sellers and Company the following year. The story of a demonstration of its action in England by one who had received a sample instrument from France is graphically told as follows:

I set to work at once, and by good luck coupled up the correct pipes to their proper flanges, but

was a great deal bothered what to do with the overflow flange. After a few nights' work I got my Injector fixed and got up steam, and to some extent began clumsily experimenting as the pressure rose to 60 pounds, the full working pressure of the boiler. I had the Injector fixed over a tank fed by a ball-tap and closed by the boiler. I turned steam on and was staggered by the rush of water into the tank from the overflow pipe, and thought something was wrong. However, I continued to turn the steam spindle, and the escape from the overflow sensibly diminished until I could turn no further. In the meantime the ball-tap started running furiously into the tank, showing me that water was going somewhere, and I knew it could go nowhere else but into the boiler. I then began to operate with the four-thread screw at the side, and found that it adjusted the water supply, and succeeded in getting the overflow "dry." I then opened the peep-holes opposite the space between the combining and the receiving nozzles, and saw the white steam passing from one to the other on its way to the boiler. I then ceased operations, and had a pipe of tobacco, . . .

The second chapter deals with the development of the principle of automatic regulation, by the adoption of which the injector was made to adjust itself automatically to conditions imposed by changes in steam pressure. The evolution of the various devices, which have been employed in the accomplishment of this function, is well set forth. Following this are several chapters dealing with the elements of design underlying each of the more important details of the injector, such as the delivery tube, the combining tube and the steam nozzle. These chapters, while constituting the more technical portion of the volume, are nevertheless so clearly expressed that the reader emerges from them with interest undiminished. A chapter entitled "The Action of the Injector" presents an analysis of the action of the entire instrument with numerical examples. It constitutes a basis for the design of such instruments, and it supplies the means for determining what are the limiting factors under conditions that may be prescribed or assumed. The longest chapter in the book, entitled "Applications of the Injector" presents excellent descriptions of the different

well-known types of injectors now obtainable, with some discussion as to their adaptability to the requirements of different service. Another chapter discusses methods of determining the size of an injector and methods of testing, and presents data derived from tests. A chapter on the requirements of modern railway practise deals chiefly with matters affecting repairs and renewals, and a final chapter discusses certain problems which arise in practise, in connection with the use of injectors in locomotive service.

At a single point only does it appear that the author slips and this is when he discusses a detail in locomotive practise rather than one affecting injector design or operation, and when a book, as a whole, is strong and true, it is perhaps ungracious in the reviewer to call attention to half a dozen lines which are in no way essential to the purpose of the book and which constitute, in fact, no more than an unguarded suggestion. There are other relations, however, in which the statement becomes one of some importance, and consequently it should not go unchallenged. Under the head of "Feeding Locomotive Boilers," the author advises that "in approaching a station at which a short stop is made, especially between long and fast runs, it is advantageous to stop the injector a short time before the station is reached, to permit a slight checking of the fire, and then, when the station is reached, to feed the boilers quickly with one, or even with both injectors if necessary, to prevent blowing off at the safety-valve." The practise here outlined is one which has been often suggested and sometimes practised. It is, however, objectionable from several points of view, and as a practise should not be tolerated. The water in a locomotive boiler when the throttle is closed is in a quiescent state. Feed water entering under these conditions is not as rapidly mixed with the water already in the boiler as it is when the throttle is open and the process of ebullition is active. As a consequence the feed entering the boiler while the locomotive is stopped at a station, being comparatively cool, settles in the lower portions of the boiler,

where it cools the metallic parts with which it has contact, and by so doing induces strains which complicate the problem of boiler maintenance. Moreover, a locomotive which is thus filled is not in good condition for the start, notwithstanding the fact that the gauge may show full working pressure; for at the start there is imposed upon the boiler the double task of supplying steam and of raising to the maximum temperature of the boiler the water which was fed into it during the stop. The result is that the boiler pressure soon falls, and considerable time is required in which to restore it.

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SOIL PRODUCTIVITY

IN a discussion of the "Secular Maintenance of Soils" before the Geological Club of the University of Chicago on January 9, the undersigned expressed views as follows:

That the era of soils began at an early but indeterminate period in the history of the earth; that the Proterozoic lands were probably mantled by soils and clothed with vegetation; that soils certainly prevailed on the land in the Paleozoic era; that sufficient soils and vegetation mantled the earth through all later eras to support the continuous evolution of land life; that the total eon of productive soils may be assigned a period of at least tens of millions of years; that therefore there must be some efficient natural process for the maintenance of soils.

That the origin of the soil body lies chiefly in the granulation of rock; that soils are wasted at the surface by wind and wash; that wind and wash also distribute granules and mix soils and give to nearly all soils some of the essential soil constituents; that progressive granulation of rock adds soils below; that progressive solution removes soil matter from soils and from the rock beneath; that by these composite processes the body of the soil is at once enriched and impoverished; that so long as the *body* of the soil is maintained, any impoverished or anemic condition that may arise can be rectified; but if the *body* be lost,

its restoration is tedious, laborious, or expensive.

That the film-water that surrounds the granules of the soil when in a normal moist state is the specific soil water; that this is to be distinguished from the ground water that lies below the water-table, though these grade into one another; that the soil swells with the growth of the films in thickness; that there is an optimum of film-water when the soil is most swollen; that addition of water beyond the optimum destroys the surface tension of the films and leads to the shrinkage of the soils, the packing of the granules and to unproductivity;¹ that the solutions in the film-water are formed with facility because of the greatness of the surface contact relative to volume; that the concentrations of the solutions are controlled by the laws of equilibrium.²

That the soil air is inversely proportional to the soil water approximately; that the soil air is to be distinguished from the earth's atmosphere, though grading into it and interchanging with it through diffusion and soil breathing; that, occupying the spaces between the film-coated granules of the soil, the soil air has great relative contact; that it acts at special advantage on both films and granules; that the union of minutely granulated earth, film-water under tension and interstitial air gives a combination of exceptional solvent and reactive power.

That the soil is the home of minute life, plant and animal; that these intensify and modify the inorganic activities; that the forms of life are with little doubt more or less predatory and parasitic on one another; that these relations are probably in some cases pathogenic, and that these give rise to unsanitary states of the soil which affect its productivity; that progress is being made by Whitney and his associates in the discovery of toxic exudations that affect productivity; that plant societies are perhaps in part a result of mutually beneficial relations in respect to exudations and by-products; that the soil thus is little less than a world in itself; that its

¹ Cameron, *Journal Physical Chemistry*, 1910.

² Cameron, *loc. cit.*

productivity is measured more by the efficiency of its complex of activities than by any mere measure of its inorganic constituents.

That the capillary cycle, a sub-factor of the drainage cycle, is an important agency in maintaining the supply of potash and phosphorus in the soils; that the selective action of clays and of ferric oxides aid in a specific way the concentration of potash and phosphorus surfaceward; that at 592 localities in France analyses showed 68 per cent. of the surface soils to be as high or higher in phosphoric acid than the subsoils, and 47 per cent. as high or higher in potassium than the subsoils, and similar facts are observed in America;³ that the phosphate rocks in the sedimentary formations are largely *secondary concentrations*; that the formation of ferric and aluminic phosphates is a phase of concentrative action; that some of the phosphoric and potassic compounds are to be grouped with the silica and the aluminic and ferric oxides as the rock-elements that tend to stay in the soils, while the compounds of soda, lime and magnesia are more liable to go down to the sea, and the carbon and nitrogen to go off into the air; that these capillary and selective actions jointly are efficient factors in productivity; that Cameron's recent estimate⁴ probably lies in the direction of the facts of the case, though confessedly only a tentative estimate based on elements not fully determined at present, viz., an annual drainage loss for the area of the United States of about 3,500,000 tons of potassium and 1,200,000 tons of phosphoric acid (PO_4); a possible crop-removal (reckoned at 1 ton per acre for the *entire* United States, carrying 1 per cent. K, and 0.6 per cent. PO_4) of 24,000,000 tons of K and 14,000,000 tons of PO_4 , while, on the other hand, the capillary waters are carrying *toward* the surface 48,000,000 to 100,000,000 tons of K and 18,000,000 to 40,000,000 tons of PO_4 .

That the plant-cycle cooperates with the capillary cycle in concentrating potash and

phosphorus toward the surface by carrying these up into the plants whence they are deposited on the surface or in the soil; that the well-known rotation of legumes and cereals that enriches the soil in nitrogen may be supplemented by a *long-period rotation of trees and annuals* for the enrichment of the soil in potash and phosphorus.

That the capillary cycle and the plant cycle conjointly contribute to a potash cycle and a phosphorus cycle by which these rise from the depths, pass into the plants, are shed as leaves, fruit and dead fiber on the surface—or pass through animals and are ultimately deposited on the surface—thence reenter the soil and are again taken up by plants, and so continue in the cycle until some intervening agency bears them out of it; that the length of this cycle is indeterminate and, in the absence of intervention, theoretically indefinite; that it is not, in the main, the *material substance* of the soil that is needed for food but *the energy* locked up in the grains, fruits, and so forth, by the anamorphic processes of the plants; that the real food comes chiefly from the sun and the material substance that temporarily embodies it is returnable to the soil indefinitely to be used again and again; that the really vital thing is the promotion of the cycle formed by plant anamorphism (solar energy going in) and animal katamorphism (solar energy coming out); that the contingencies of loss lie chiefly in the removal of the katamorphic products before they again enter into a new anamorphic process, contingencies that man emphasizes.

That the SzeChuanese of West China, occupying a hilly sub-mountainous sandstone region whose area is less than that of Texas, a people numbering 68,724,800 according to the Chinese census, embracing more farmers probably than does the entire United States, have cultivated their soils continuously from an undetermined date before the beginning of the Christian era and quite without rock phosphates apparently, and yet have maintained a productivity exceeding, area for area, that of the virgin soils of America; that with little doubt this fertility can be maintained

³ Bureau of Soils, U. S. Department of Agriculture.

⁴ *Journal of Physical Chemistry*, 1910.

by the present mode of treatment until the country is base-leveled; that the SzeChuanese have thus demonstrated one mode of effective secular maintenance of the soil productivity; that their method is closely analogous to the natural method of the geologic ages; that a Chinese expert would criticize western practice as influenced unduly by prejudice respecting the use of the katamorphic products of human food-consumption.

That notwithstanding the loss due to this prejudice respecting the use of human katamorphic products, the soils of western nations generally show increases of productivity in the later years compared with the earlier; that, in particular, the data furnished by the Bureau of Statistics and the Bureau of Soils that the productivity of the soils of the United Kingdom, France, Belgium, Netherlands, Denmark, Germany, Austria, Hungary, Roumania and Russia show rather steady and notable increases in productivity for the last two or more decades that are covered by the statistics; that the lands most densely inhabited and most intensively cultivated, such as those of England, France, Germany and neighboring states, are more productive, unit for unit, than those of Russia, which are less densely occupied and less closely and persistently cultivated; that the old soils of Europe are more productive, unit for unit, than the newer soils of America; that in the United States the productivity of the last forty years shows general increase per acre; that the increase per acre in the older states, as the New York-New England group or the middle states, is more marked than in the southern or in the western groups, notwithstanding the larger proportion of virgin soil recently brought under cultivation in the last group; that while these and all similar statistics are subject to many qualifications in interpretation and application, they do not offer substantial grounds for an alarming forecast, applicable to an industrious and intelligent people willing to be guided either by oriental experience or by western scientific research.

T. C. CHAMBERLIN

UNIVERSITY OF CHICAGO

NOTES ON METEOROLOGY AND CLIMATOLOGY

THE effect of the recent construction of high buildings in New York City upon the United States Weather Bureau's records of wind velocity and direction for that city are discussed by Mr. E. S. Nichols, the local forecaster, in the October number of the *Monthly Weather Review*. Since the anemometer and the windvane were placed upon the American Surety Building at an altitude of 350 feet above the street in 1900, several new "skyscrapers" have been erected in the immediate vicinity, vitiating to a greater or less extent the wind records since obtained. A comparison of the bureau's records with those of the New York Meteorological Observatory in Central Park, where the environment has not been greatly changed in forty years, shows that there has been a decrease of 16 per cent. in the hourly wind movement directly attributable to the recent construction. North winds have been affected the most; northeast and east have not been changed materially; while other directions have been considerably reduced. The number of days upon which gales have been recorded has decreased noticeably, and wind direction has been more or less deflected. Partly because of a desire to prevent the recurrence of such a condition in other cities, the bureau is gradually erecting appropriate buildings of its own in localities where future changes in the environment are not likely to affect the records obtained.

FROM an investigation of the relation between solar activity and terrestrial temperatures, Professor Humphreys has come to the conclusion that the decrease in the ultra-violet radiations received by the earth during the period of sun-spot maximum causes a similar decrease in the amount of ozone formed in the upper part of the earth's atmosphere. Moreover, since ozone allows the solar heat rays to penetrate it freely but absorbs most of the returning earth reflection, spot maxima indirectly produce diminished terrestrial temperatures. Abbot and Fowle had already concluded that spot maxima are accompanied by terrestrial temperature minima, and *vice*

versa, but had not recognized the significance of ozone in the sequence of events. The revival of scientific interest in ozone dates from 1904, when the late Professor Angström showed that a large amount of it existed in the upper atmosphere. As much may be gained from a further study, the chief of the Weather Bureau has urged the International Meteorological Committee to investigate the problem.

THE latest publication of the English Solar Physics Committee, "Southern Hemisphere Surface-air Circulation," was prepared by Dr. William J. S. Lockyer, secretary of the Solar Commission of the International Meteorological Committee. The work consists of a study of the mean monthly pressure amplitudes, the tracks of the cyclones and anticyclones and the meteorological records of several Antarctic expeditions. In an earlier memoir Dr. Lockyer pointed out the apparent similarity of the air movements over Australia, South Africa and South America, and suggested that anticyclones which crossed Australia were indications of a continuous state of things occurring in a belt encircling the earth. In the present memoir he shows the presence of such a belt in which movement is from west to east. The survey, which is an extensive one, doubtless will aid in the attempt to associate solar activity with the air movements of the southern hemisphere. Moreover, it also suggests that greater importance, from this point of view, must be attached to the meteorology of the polar regions than has hitherto been the case.

FROM a study of simultaneous records made at Corona, Colo., altitude 11,660 feet, and Denver, Colo., altitude 5,347 feet, distant in an air line about 38 miles, Professor A. J. Henry arrives at conclusions which briefly stated are as follows: (1) In general the temperature changes at high and low level stations are nearly synchronous, in point of time, and similarly directed. (2) Any abnormal course of the temperature between a mountain station and a near-by low-level station can generally be explained by considering the pressure distribution over the surrounding regions to a distance of at least 1,000 miles

from the station. (3) An inversion of temperature between Corona and Denver occurs most frequently when the latter is under the influence of a Montana anticyclone while the former is affected by a cyclone to the west. (4) The high southwest and west winds occasionally observed on Pikes Peak and Corona indicate the early formation of a cyclone to the northwest or north. (5) In winter, mountain temperatures fall whenever a cyclone passes eastward across the mountains, or southeastward from Montana to Kansas. (6) The temporary presence of an anticyclone in the Great Basin affects the winds upon the mountains of central Colorado, giving high temperatures and fair weather. (7) The latter mountains cause a slight lowering of the pressure in an anticyclone as it passes over them.

ACCORDING to the Bulletin of the Mount Weather Observatory issued by the Weather Bureau October 31, 1910, during the three years in which regular free-air observations have been made the kite flights over 5,000 meters above sea-level number 31. Of these, three are over 7,000 meters, while in six of the flights the kites flew at a greater altitude above sea-level than has been attained elsewhere. The flight of 6,440 meters made April 5, 1910, at the Royal Aeronautical Observatory, Lindenberg, Germany, is the seventh highest above sea-level. In the opinion of Dr. William R. Blair, who has charge of the aerial work, the kite-flying apparatus has usually been the limiting factor at Mount Weather, and as this is gradually being improved, he expects that the kites will attain still greater heights. The upper air data are not only used by the forecasters in the central office in Washington, but it is hoped that when interpreted they will add to our knowledge of the atmosphere as a whole. Owing to the nearness of the ocean sounding balloons are not liberated on Mount Weather, but they have been sent up periodically from Indianapolis, Ind., Fort Omaha, Neb., and Huron, S. D. As these experiments have resulted in the acquirement of very desirable data, it is probable that they will be continued and perhaps extended during the present year.

A PRELIMINARY report of the investigation of the upper air in Java has recently been made by Dr. W. van Bemmelen and Dr. C. Braak. Aerological investigation at the Batavia observatory was begun under the auspices of the Dutch government in 1909. Because of the proximity of the sea, pilot balloons only were used at first, and with these a more thorough knowledge of the upper currents was obtained. Later recording instruments were elevated by means of captive balloons and kites, the latter being used above the sea as well as above the land. It was found that during the period September-May the general air-current had easterly components up to the greatest heights attained (10-15 kilometers), though occasionally the west monsoon appeared at the ground, its average height having been found to be 5.4 kilometers. No antitrade wind aloft was found. However, on one occasion when a balloon attained a height of 18 kilometers it encountered a westerly wind, similar to the strong westerly winds which were observed at heights of 10-20 kilometers on Professor Berson's East-African expedition. This phenomenon still awaits an explanation.

THE newly created professorship in meteorology at the National University at Utrecht has been awarded to Dr. E. van Everdingen, who assumed the chair October 17. Considering the recent history of meteorology, the inaugural address, "The Third Dimension in Meteorology," was particularly appropriate. In Dr. Everdingen's estimation, the setting apart of a chair of meteorology indicated a recognition that meteorology was now worthy of a place among the established sciences.

As a result of many requests from teachers, students and others interested in the subject, the Weather Bureau has published a second compilation of standard books dealing with meteorology and its several branches. The list includes about 150 titles, the selections having been made by Mr. C. Fitzhugh Talmán, librarian of the bureau. As stated in the introduction, "the present compilation is the fruit of several years' experience in dealing with the literature of the subject, and will

probably meet the requirements of the majority of American readers and students."

THE action of the management of the recent International Aviation Meet at Belmont Park in taking out insurance against loss due to inclement weather is one of the first instances of its kind in America. The practise is a common one in Europe, however, especially so in England, where managers of most of the outdoor gatherings have long insured through Lloyd's against loss from wet weather. The premiums paid for the risks were relatively large at first, but of late there has been a tendency toward placing the practise upon a scientific basis, statistics having been gathered with that end in view, and in consequence the rates have been readjusted.

ANDREW H. PALMER

BLUE HILL OBSERVATORY,
January 14, 1911

SPECIAL ARTICLES

INTERPRETATIONS OF RESULTS NOTED IN EXPERIMENTS UPON CEREAL CROPPING METHODS AFTER SOIL STERILIZATION¹

It is not my intention at this time to give the details of extended experiments upon soil sterilization and its effects; nor to enter any special criticisms upon the work of other investigators. I wish only to call attention to some facts, observations and conditions of the work centered about cereal cropping and experiments upon soils which may indicate that a new light may be thrown upon the conclusions to be drawn; with that light emanating from a different source than has usually been indicated by most experimenters.

Observations and Reflections.—The following features of cropping and experiments will be familiarly known to most of you:

1. New Lands, when first sown to wheat or other cereals, produce quite lavishly in seed of high quality and at slight effort on the part of the farmer. These new land yields, in this country, are quite commonly taken as the standard of what ought to be expected.

¹Read before the Society of Agronomy, Washington, D. C., November meeting, 1910.

2. It is a common experience that as soon as a particular cereal crop has become general, and that usually follows in a very few years, a marked deterioration, both in yield and quality, sets in. The crop, except in special years, and under rare exceptions of special farming, seldom again reaches the same high grade of yield and quality. Indeed, the yield generally falls to the average for the country, above which it can be raised again only through exceptional methods; and, to the chagrin of many of our most able agricultural educators, no philosophy of cropping or land improvement seems to give the farmer the desired results with any regularity, year by year, for any long period of time. The crop or variety once a favorite in a locality usually has a short life and finally gives place to a real change in agriculture, seldom, if ever, to regain its place.

3. Not many theories have been advanced to account for these results. The chemist and his followers have usually directed thought in the matter, and agriculturists, generally, have taken the chemist's dictum that marked changes have occurred in the balance of plant food relations of the soil, thus accounting for the rapid first deterioration of the crop through chemical losses noticed in the soil. Thus if a lack of proteid is found in the grain of wheat and a loss of nitrogen is observed in the soil, it has been reasoned, without foundation, I think, that the noticed chemical loss in the soil is necessarily the cause of the deficiency in the kernel. When our chemical friends have, by their own analysis, discovered that there is, however, sufficient strength of soil solution regarding all known necessary chemical elements to support a crop on a particular field, the failure to reach crop quality has been quite uniformly attributed, by them and the rest of us, to slovenly methods of farming, poor physical texture of the soil, degenerated seed, etc.

Any other special theories which have been advanced in particular to account for the facts have all been strongly influenced by the recognized fact that soil can be impoverished, re-

duced in its chemical strength. The Whitney toxine theory would appear to be only a reflection of this troubled state of the chemical and physical mind, associated with a desire to show that a complex plant growing in the soil and air acts upon the soil after the manner of a bacterial culture in a test tube. That I may not be misunderstood, I may say that I believe that certain soils may be exhausted chemically by cropping methods; that I think it is wholly possible that the excrementia of plants under rather constant cropping may have an analogous effect upon the crop to that noted in bacterial cultures upon the substratum, but that after several years of careful trials upon wheat and flax, both under culture house conditions, and under carefully planned plot trials, I have been unable to find any point which would tend to substantiate the toxine theory. Nevertheless, the contention of Mr. Whitney, that the soils of cereal regions are not particularly exhausted is, in my belief, much nearer to the truth than the contention of the chemists and others that the deteriorated yields and qualities of wheat and other cereals are due to chemical exhaustion, and especially to nitrogenous exhaustion; for neither the chemists' exhaustion theory nor the toxine theory can account, to my satisfaction, for the failure of virgin soils to produce the yields characteristic of that region when such cereal cropping was first introduced. It is a fact that such lands are quite as liable to give the crop characteristic of the old, so-called, worn-out lands, as do the older lands. It is not the uniform failure of the particular crop which causes it to be dropped by a farming community, for it is evident that all of the lands of a community can not be so depleted. It is the general uncertainty of giving results, year by year, which results in abandoning or ceasing to expect a proper yield. It is evident from the foregoing considerations that there are constant interfering agencies at work in cereal cropping regions which have not as yet been properly taken into consideration, for, even under the best weather conditions possible, essentially the same weather conditions which in a new

land region give fine yields, often the crop fails to give both quantity and quality even under our best planned systems of rotation and of soil fertilization.

4. Experiments in soil sterilization applied to such old and supposedly deteriorated soils have uniformly given quite marked improvement in results. The results have been so uniformly good, whether done by steam or by chemical methods, that one or other practise has become general with the glass house gardeners and seedling plant producers. They seem, long ago, to have realized what sterilization of soil has done for them, but experimenters upon field crops still look for explanation for such improvements.

5. Two very interesting explanations of such effects of sterilization, both based upon carefully planned and executed experiments, have lately been attempted; and, as my experiments cover essentially the same fields of effort, and, when published, will show almost exactly the same results but quite different conclusions, I may be pardoned, at this time, for outlining these three sets of experiments and the results, with some slight comment upon the conclusions:

Mr. A. D. Hall, of Rothamsted, England, in *SCIENCE*, September 16, 1910, reports upon experiments conducted at the Rothamsted farm.

Speaking of wheat, he says:

Approximately the crop becomes double if the soil has been first heated to a temperature of 70° to 100°, for two hours, while treatment for forty-eight hours with the vapor of toluene, chloroform, etc., followed by a complete volatilization of the antiseptic, brings about an increase of thirty per cent., or so. Moreover, when the material so grown is analyzed, the plants are found to have taken very much larger quantities of nitrogen and other plant foods from the treated soil; hence, the increase of growth must be due to larger nutriment and not to mere stimulus.

The explanation, however, remained in doubt until it has been recently called up by Drs. Russell and Hutchinson, working in the Rothamsted Laboratory. In the first place, they found the soil, which had been put through the treatment, was chemically characterized by an exceptional accumulation of ammonia to an extent

that would account for the increased fertility. At the same time it was found that the treatment did not effect complete sterilization. . . .

The question now remaining was, what had given this tremendous stimulus to the multiplication of the ammonia-making bacteria? By various steps, which need not here be enumerated, the two investigators reached the conclusion that the cause was not to be sought in any stimulus supplied by the heating process, but that the normal soil contained some negative factor which limited the multiplication of the bacteria therein.

Examinations along these lines then showed that all soils contain unsuspected groups of large organisms, of the protozoa class, which feed upon living bacteria. These are killed off by heating, or treatment by antiseptics, and on their removal, the bacteria, which partially escape the treatment, are now relieved from attack. . . .

Curiously enough, one of the most striking of the larger organisms is *amœba*.

The authors, Messrs. Russell and Hutchinson, thus attempt to account for the greater wheat crop production of soil sterilization both through chemicals and through steaming, by a reverse application of the Metchnikoff theory. It would be unwise of me, not knowing all of their data or having access to the soil or the seed which they used, to enter a criticism, but from my own observations and work, I can not agree to any of the conclusions which are drawn in these paragraphs. So far as Mr. Hall has made plain in *SCIENCE*, they can only be matters of inference, and many conditions could enter, which would vitiate the necessity of assuming the detrimental rôle for the *amœba*. For example, the authors do not explain why their sterilization did not sterilize, and what happened when they did really sterilize the soil. In order to clarify the theory as proposed by Dr. Hall, it would seem necessary to try real sterilization, both upon the *amœba* and the supposedly beneficial bacteria.

It is quite possible that the production of ammonia in soils by bacteria is a beneficial process, but I can not say wherein this theory would rest, if one should assume the presence of plenty of ammonia and plenty of ordinary nitrates in the soil. In such case, if the soil still failed to produce wheat, and proper ster-

ilization succeeded in making it produce wheat, their theory would seem to be without ground. My experiments in sterilization result in either good or bad wheat according to what I do to the seed planted therein, though there can not be any question but what in some soils increased amounts of ammonia through sterilization do have something to do with the results.

Experiment by Professor T. L. Lyon, of Cornell University, Bulletin 275, "Upon the Effect of Steam Sterilization on the Water-soluble Matter in Soils," attempts an explanation of the peculiarities of growth of the wheat plants upon soils after steam sterilization through differences in the soluble content of the soil resulting in differences in density of the soil solutions, etc. He also, however, seems to have great difficulty in accounting for some of the peculiar actions of the growing wheat plant upon such treated soils and solutions, especially in explaining what appears to be a really injurious effect upon the first growth from the seedlings, though finally followed by actual increase in crop.

In our experiments, we are able to explain most of these peculiarities of growth, noticed both in our cultures and those of Professor Lyon's admirably conducted trials, upon a biological relation of the wheat plant to certain actual disease-producing organisms and their growth relations to the crop plant, and to the various interreacting soil relations, which react both upon the crop plant and upon the disease producers.

In our experiments we find that both soil and seed may be, and usually are, infected by several very destructive wheat-destroying, parasitic fungi. Indeed, these are found to be apparently cosmopolitan in distribution with the wheat plant. They are especially transmitted in the seed internally, and, it seems quite certain, are sufficient in their influences to account for most of the causes of rapid first-crop deterioration, and for the difficulty which farmers have in introducing any sort of culture, which will again raise the standard of crop. Their exclusion, in so far as it is perfectly or imperfectly done, is suffi-

cient to account for the anomalies indicated in soil sterilization experiments. However, in our experiments our results and conclusions have always been vitiated whenever these fungi were not eliminated.

I do not question that soil sterilization does change the bacterial content or that it does influence the soluble content of soils, but I am inclined to think that disease-infected seed and disease-infected soil will eventually be found to have much more to do with the irregularly corresponding conclusions, which have been drawn by various experimenters upon crop rotations, upon soil-fertilization experiments and upon soil-disinfection experiments than they have ever suspected. Indeed, I have but slight doubt that the whole theory of auto-intoxication (toxine theory) as applied to cropping plants, is virtually vitiated in its conclusions, because of a lack in eliminating the influences of pathogenic organisms in the experiments.

H. L. BOLLEY

AGRICULTURAL COLLEGE,
NORTH DAKOTA,
November 1, 1910

TERTIARY DEPOSITS OF NORTHEASTERN MEXICO

DURING the past two years, the geologic work under my direction in southwest Texas and northeast Mexico has resulted in the accumulation of a mass of information which materially adds to our knowledge of the Gulf Tertiaries. The fieldwork was carried on by Messrs. W. F. Cummins and W. Kennedy, assisted by Mr. J. M. Sands.

The first year's work by Professor Cummins was a general examination of the northeastern Mexico for artesian water. Following this, I had a careful section made of the Cretaceous and Tertiary deposits along the Rio Grande, and then traced the contact between the two systems southward into Mexico as far as this could be done within the scope permitted by our economic work. The widespread occurrence of the different phases of the Reynosa formation prevented direct connections of the exposures of the underlying deposits in some places, but we were able to

carry the Cretaceous-Tertiary parting with a fair degree of accuracy from the actual contact at the Arroyo del Caballero on the Rio Grande to a similar contact at Ramones, forty miles east of Monterey, and from Panalito on the Conchos River to the southern boundary of the state of Tamaulipas. We hope to fill the gap between Ramones and Panalito before we finish.

Numerous sections were made of the overlying Eocene to the eastward of this line of contact and good collections of fossils were made from various horizons in it, which prove that the substages recognized in Texas continue south as far as we found any deposits of this age. We were able to map approximately the areal distribution of each of these.

The highly fossiliferous deposits on the Rio Grande which constitute the upper member of the Cretaceous of that region and which are known as the Escondido beds, only continue southward in this character for forty miles to a point southwest of Laguna de la Leche, where they are covered by much later deposits. Where these later deposits end near the Salado river west of Rodriguez we find underlying them, in place of these fossiliferous beds, beds of blue shale without fossils, which have the same relation to the overlying Tertiary that the Escondido has through its whole extent. These blue shales, which we call the Papagallos shales, are therefore considered to be the extension or equivalent of the Escondido and are found to extend south to the limit of our investigations. To the south, these blue shales draw nearer and nearer to the coast until at their crossing of the Zarziza in southern Tamaulipas there is barely ten miles of Tertiary deposits between the outcrop of the shales and the waters of the gulf.

On the Rio Grande and at several other points between that stream and the Pesqueria we found deposits of Midway age, as proved by its characteristic fossils, resting directly on the Cretaceous, but for the most part this basal contact is covered or obscured by the overlapping Carrizo sand. The last of the Midway was seen at Ramones.

When we again pick up the contact at Panalito on the Conchos River, we find both the Carrizo sands and Marine beds in contact with the blue shales of the Cretaceous, while a few miles down the river the Yegua overlaps both of these and is the substage in contact almost to the Soto la Marina. A few miles north of this river, the Yegua, Fayette and Frio are in turn covered by the San Fernando beds and beyond that point we found no further exposures of the Eocene.

South of the Soto la Marina River the beds of the Eocene seem to be entirely wanting, if they were ever laid down. Our drilling records as yet show no evidence of them, unless some part of the blue shale on which the San Fernandan rests be proved later to be of Eocene age. No fossils have yet been found in this shale, but its general character and relations to overlying Tertiary as well as to the Escondido and to other known Cretaceous deposits seem to warrant its reference to the latter period.

The San Fernando beds, which are regarded as Oligocene, were first observed at San Fernando on the Conchos or Presas River and have an extensive development to the south, entirely overlapping or replacing the Eocene deposits and resting directly upon the Cretaceous. This formation, with its beds of nummulitic limestone and great numbers of cristellaria, carries an extensive and varied fauna and has a much greater development than previously observed. It is succeeded by other beds of similar composition, but of distinctly later age, which in turn overlap it and reach the underlying Cretaceous shales.

These later beds continue down the coast as far as Tecolutla. They are well exposed at Tuxpam, where they have a highly characteristic fauna, including two very heavy oysters which are nearly round. Both are of the same general shape, but one of them has on one valve four or five deep plications. The echinoderms of these beds are especially noteworthy, there being probably eight species in our collection. The most abundant form is a *Clypeaster* which attains a diameter of

seven inches and which occurs in great numbers in the yellow sands around Tuxpam.

There are also casts of a large variety of other forms of bivalves and gasteropods, and as a whole the fauna is later than that of the San Fernando beds and is probably Miocene. We have called these the Tuxpam beds.

The evidence now before us indicates that the upper Tertiary deposits mark a gradually sinking coast line along the gulf border in Texas and Mexico which was arrested in the Tampico-Tuxpam region before it was further north. Thus while early Miocene deposits are on the surface almost at the present water's edge at Tampico and have only a small depth of later deposits overlying them, deposits of the upper Miocene are buried 2,300 feet on Galveston Island and are found in drilling at Saratoga seventy miles inland at a depth of over 1,000 feet.

E. T. DUMBLE

SOCIETIES AND ACADEMIES

THE AMERICAN PHILOSOPHICAL SOCIETY

IN an address on modern physics before the American Philosophical Society, recently, Professor Ernest Fox Nichols, president of Dartmouth College, said in part:

"I shall try to review very briefly the principal ideas upon which modern physics rests and shall say something about where we think we have arrived in our search for knowledge. I need scarcely remind you that in the natural sciences as in more practical affairs, *how* we have arrived is as important as *where* we have arrived. I shall therefore spend some time in presenting detached fragments of the experimental evidence and inferences upon which certain conclusions are based, hoping in this way to illustrate some of the constructive methods of reasoning employed in research.

"The ideas which underlie all our thinking are space, time and inertia or mass. With space and time as a background, the physicist must pursue inertia and everything related to it, along every conceivable path. In this pursuit he comes upon four ultimate though related conceptions: matter, ether, electricity and energy.

It should be remembered that an important part of our present knowledge of matter, and nearly all that we know of ether and electricity, has been gained not immediately but by inference.

In so many cases we see or know directly only the first and last link of a chain of events and must search by indirect means for the mechanism lying between.

"At bottom, I suppose, the ether, electricity, force, energy, molecule, atom, electron, are but the symbols of our groping thoughts, created by an inborn necessity of the human mind which strives to make all things reasonable. In this reasoning from things seen and tangible, to things unseen and intangible, the resources of mathematical analysis are applied to the mental images of the investigator, images often suggested to him by his knowledge of the behavior of material bodies. This process leads first to a working hypothesis, which is then tested in all its conceivable consequences, and any phenomena not already known which it requires for its fulfilment, are sought in the laboratory. By this slow advance a working hypothesis which has satisfied all the demands put upon it gradually becomes a theory which steadily gains in authority as more and more new lines of evidence converge upon it and confirm it.

"As we take up what we believe to be the relations of electricity to matter, we come in places upon slippery ground and the bases of our faith rest on recent foundations.

"At the outset we encounter one striking difference between electricity and matter. Every free charge of electricity exerts a force upon every other charge in the universe, just as every particle of matter exerts a force on every other particle of matter, however distant. But with matter the particles are invariably urged toward each other while electric charges may be either drawn together or forced apart depending on the kinds of charges. We have both positive and negative electricity but only one kind of matter. The bald statements of the laws of gravitation and electric force bear a strong resemblance to each other. The laws tell us how the forces *vary*, but reveal no hint of the machinery by which they *act*. Of the intimate association of electricity with matter we have learned much from careful study of the processes of electric conduction in solutions and gases."

The contributions to our knowledge gained from the recent discoveries of cathode rays, X-rays, spectroscopic studies and the amazing properties of radio-active substances were next discussed and in closing Dr. Nichols said:

"The electron has but a thousandth part of the inertia of the lightest known material atom, and this inertia it doubtless borrows from the kindly

ether and does not hold in its own right. Its behavior is that of an atom of negative electricity pure and simple. Its form is spherical and not spheroidal. Its size is probably less than one ten-million-millionth of an inch. When revolving briskly enough in an orbit within the atom it gives us colored light of highest purity. When violently jostling irregularly about it gives us white light, without it all light would be impossible.

"We believe we have found electricity free from matter but never yet matter free from electricity. Finally comes the suggestion that matter no less than life may be undergoing a slow but endless evolution. Some of these things and many others have led physicists to suspect that if all electricity were removed from matter nothing would be left, that the material atom is an electrical structure and nothing more.

"There are, however, many stubborn questions to which answers must somehow be found before the so-called electron theory of matter can be accepted unreservedly. As it stands it is at once a most brilliant and promising hypothesis but has not yet reached the full stature of a theory.

Should it hold good the material atom with its revolving electrons becomes the epitome of the universe. The architecture of the solar system and of the atom, the very great and the very small, reveals the same marvelous plan, the same exquisite workmanship. The conservation of energy becomes an ethereal law and the ether the abiding place of the universal store of energy.

"To end as we began, we have matter and electricity which some day we may know to be one, and ether and energy. Of these we hope some time to build, in theory, a reasonable world to match the one we now so little understand.

"When all the interrelations among matter, ether, electricity are separated out and quantitatively expressed, we believe our work will be complete.

"Such then is the confession of faith, the very far distant hope of the modern physicist."

November 1, 1910

THE PHILOSOPHICAL SOCIETY OF WASHINGTON

THE 687th meeting of the society was held on January 14, 1911, President Day in the chair. Two papers were read:

Discrepancies among Recent Wave-length Determinations: I. G. PRIEST, of the Bureau of Standards.

The speaker, in introducing the subject, reviewed briefly the history of spectroscopic standards, stated that the accuracy at present desired is about 0.001\AA , and gave a brief description of the "method of diameters."

In regard to the discrepancies among the results of Fabry and Buisson, Pfund and Eversheim,¹ the following conclusions were presented and supported by tabular data.²

1. The difference $(F. \& B.)_{.08} - P_{.08}$ is not markedly systematic when the whole range of the spectrum covered is considered. Considering the precision of the measures, the systematic difference that does appear is perhaps negligible.

2. Throughout the range of the spectrum from $5,167\text{\AA}$. to $6,495\text{\AA}$., inclusive, the difference $(F. \& B.)_{.08} - P_{.08}$ is sensibly systematic, the algebraic mean discrepancy being $+0.0015\text{\AA}$. Out of the total of twelve differences to be considered in this range, only one is negative, viz., -0.001\AA . for $\lambda = 5,167\text{\AA}$., the limit of the range.

3. Throughout the range of the spectrum from $4,282\text{\AA}$. to $5,002\text{\AA}$., the difference $(F. \& B.)_{.08} - P_{.08}$ is not *markedly* systematic, although there is a slight predominance of negative values, the algebraic mean discrepancy being -0.00045\AA . Out of the total of eleven differences to be considered, four are positive, five are negative and two are zero.

4. Considering the whole range of the spectrum covered in common by the several investigators, the results of Eversheim appear to be systematically higher than those of Pfund and Fabry and Buisson by about 0.001\AA .

5. The differences, $(F. \& B.)_{.08} - E_{.08}$ and $P_{.08} - E_{.08}$, when grouped according to sign are also grouped in certain spectral regions as indicated in Table I. In the differences $P_{.08} - E_{.08}$ the coincidence of the grouping according to sign and the grouping in spectral regions is pronounced and unmistakable. The spectral grouping of the positive and negative differences $(F. \& B.)_{.08} - E_{.08}$ while less pronounced than for the differences $P_{.08} - E_{.08}$ is not consistent with this grouping, and the tendency of the groups in the system $(F. \& B.)_{.08} - E_{.08}$ to coincide in spectral position with groups of the same sign in the system $P_{.08} - E_{.08}$ is decided.

¹*Astrophys. Jour.*, **28**, 195; J. H. Univ. Cir., Feb., 1910, pp. 33 and 34; *Ann. der Phy.*, **30**, pp. 837-838.

²Initials and subscripts refer to authors and year of publication. See also *Phys. Rev.*, **31**, 602.

Table I³

Group	(F. & B.) ₀₈ —E. ₀₉					Algebraic Average of Differences
	Limits of Group	Number of Lines in Group	Distribution of Differences According to Sign			
			+	0	—	
I.	4,282 4,593	9	4	3	2	0.0000
II.	4,603 5,083	16	1	1	14	—0.0022
III.	5,110 5,456	10	5	3	2	+0.0016
IV.	5,498 6,495	17	3	5	9	—0.0014
	P. ₃₈ —E. ₀₉					
I.	4,282 4,494	5	5	0	0	+0.0016
II.	4,860 5,002	5	0	0	5	—0.0020
III.	5,167 5,456	4	3	0	1	+0.0022
IV.	5,498 6,495	8	0	0	8	—0.0038

The large discrepancies existing between Eversheim's determinations in the helium, cadmium and mercury spectra⁴ and the earlier results of Rayleigh, Michelson, and Fabry and Perot were mentioned.

As conditions of apparent significance in connection with conclusions 2 and 3 above, the two following facts were emphasized:

1. The line of division between the group of wave-lengths considered in (2) and that considered in (3) is sensibly coincident with the green cadmium reference wave-length.

2. Fabry and Buisson's and Pfund's correction curves for "dispersion of phase" cross each other at about this same wave-length.

As a tentative hypothesis to account for conclusion 4 above, the speaker suggested an insufficient approximation in computing. In support of this hypothesis, he stated that he had recomputed from the published data, the results under II., p. 836, Vol. 30, *Ann. der Physik* (1909), and obtained values systematically lower than the ones there published. These recomputed results

³ Data from same sources as mentioned in footnote 1.

⁴ *Zs. für wiss. photog.*, 8, 148, March, 1910.

were obtained on a ten-place computing machine and so involved no approximation in computation. Computation by seven-place logarithms gave results systematically high, while computation by eight-place logs checked the machine results. It happens that the errors of the seven-place table are additive instead of compensating in this case, so that the error in the final result may amount to +0.002Å. There is a possibility of the approximate logarithmic computation introducing a systematic error owing to the fact that all wave-lengths are referred ultimately either to the green or red cadmium wave-lengths as standards; and to the fact that the values of K (see p. 835, Vol. 30, *Ann. der Phys.*) nearly enough equal to fall at the same point in the log table may be expected to frequently occur. As to the importance of this latter condition, nothing can be said without consulting the original data. It seems possible, however, that this condition if it occurs often enough, in connection with the error due to approximation in log λ, may cause a discrepancy about large enough to account for the observed discrepancy between the results of Eversheim and the other investigators.

Ocean Currents and Barometric Highs and Lows:

Dr. W. J. HUMPHREYS, of the United States Weather Bureau.

In the first part of the paper the speaker dealt with the five barometric highs on the oceans which remain substantially fixed in position throughout the year though varying in intensity, three of which are in the southern, and two in the northern hemisphere. In the second part of the paper the speaker discussed the Aleutian and the Icelandic regions of low barometric pressure.

A brief review was given of the explanations advanced by past investigators to account for the existence and character of these regions of high and low barometric pressure, none of which appeared adequate to account for all of the observed facts.

Lantern slides were exhibited showing the isobars, isotherms and ocean currents over the ocean areas, and the relation of these to the existing high- and low-pressure areas was discussed at some length, the purpose of the paper being to show the physical reasons for the existence of these highs and lows and to explain why they are where they are actually observed to be. (This paper will appear in full in an early number of the Bulletin of the U. S. Weather Bureau.)

R. L. FARIS,
Secretary